

Interviews on the Development of *The River Continuum Concept*

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INTERVIEWS ON THE DEVELOPMENT OF THE RIVER CONTINUUM CONCEPT WITH THE AUTHORS OF THE 1980 ARTICLE THAT APPEARED IN THE *CANADIAN JOURNAL OF FISHERIES AND AQUATIC SCIENCES*

The five authors (Robin Vannote, James Sedell, Colbert Cushing, Wayne Minshall, Kenneth Cummins) of “The River Continuum Concept” came to The University of California at Berkeley from 24-27 February 2005 to be interviewed and videotaped about the development of this new paradigm in stream ecology. Group discussions that were taped extended over 25 and 26 February, and individual interviews with each of the authors were held during this period as well. The proceedings were taped by Paul Durst (University of California, Berkeley) and Michael Furniss (US Forest Service). Additional interviews were conducted with Dr. Bernhard Statzner, one of the first to comment on the applicability of the River Continuum Concept to non-North American rivers, and Prof. Luna Leopold, one of the progenitors of the original idea.

Transcripts of the interviews were prepared by Paul Durst, Susan Durst, and Elizabeth Durst from audiotapes that were simultaneously made. Transcripts were then sent on to each of the participants for correction. The individual interviews were corrected by Cummins, Minshall, and Cushing; neither Vannote nor Sedell made corrections to their interviews. The group interviews were corrected by Minshall, Cummins, and Cushing. Corrections were limited to names, places, and points of clarification; no additional material was added.

The videotapes and audio files of the interviews have been placed on two hard drives, one is available from the US Forest Service Pacific Southwest Research Station (contact is Dr. James Sedell). The other is in the Department of Environmental Science, Policy & Management, University of California, Berkeley (contact Prof. Vincent Resh). The files are stored individually on the hard drive (Introduction, 6 group interviews, 5 individual interview). The transcripts are with the authors of the article and Vincent Resh.

We thank all those who participated in this project, the US Forest Service who funded these visits, and others who assisted with logistics.

Vincent H. Resh, Professor, and Paul A. Durst, Research Assistant

Addendum added 16 June 2016

During the interviews, the question repeatedly came up about what was the purpose of these recordings and transcripts. I had intended and indicated that these were to be available for use by future scientists and historians of science. We finally agreed that these transcripts and tapes would not be released for 10 years, which was acceptable to the participants. A copy of the taped interviews is also at the Stroud Water Research Center in Avondale Pennsylvania.

Vincent Resh, Professor

Group Meeting 1

VR: One of the things that became clear at that meeting was that the River Continuum Concept was not only the most cited paper, it was three times as cited as the second most cited paper. So, one of the things that was clear was that we wanted some kind of documentation about how this idea came about, and I think that this is what we are all here for. I thank you for all coming down here, and we're really anxious to hear your thoughts about this. One of the things we'd like to start off with, this is going to have multiple inputs, is basically how did you just get to know each other? The continuum was maybe twenty-five years ago, but this is something that happened long before that, your initial contacts with each other.

Robin Vannote: I think, most certainly, professional meetings are the principal interaction of initial contact. Also, graduate school ties, professor ties and interactions, along those lines. I think the River Continuum group that's here, it was the Boston meeting. I think Ken was the organizer of that one.

Ken Cummins: Yeah. Triple A S

RV: Michigan State University put those talks together.

Bert Cushing: Four of the people here gave papers at that first Boston one.

VR (1:39): Did you guys know each other before then? Ken, how did you know the people in the group, if you could just mention?

KC: Through major professors, probably.

VR: How about Wayne?

KC: I knew about the Doe Run Work that he had done, and I knew about Robin through Bob Ball, and Cush...now how did we get started?

BC: The first real recollection that I have is the Pymatuning

KC: Oh yeah! That's right. That's where it was.

BC: And the first time I met Wayne was at the Boston one. I, of course, bonded with him, but I first met him at the Boston one. I think that was the first place I met Robin too.

Wayne Minshall: Right. And you will recall that it wasn't me that did the Doe Run work. That was Minckley

KC: Oh ok! Yeah right. What's the other one?

WM: Well, allocthonous detritus and so on. We were all recently out of graduate school, so at least I don't know if I'd met you before triple A S.

KC: I don't think so. It's all pretty fuzzy past history.

WM: And then Bert and I roomed together at the triple A S meeting.

VR (2:49): What year was the meeting?

All: 1969.

WM: So, some of my contacts were from a whole different angle than you folks. I know we started getting involved in the International Biological Program, and for some reason, we were doing an exchange with students, and we showed up at the hotel in Richland. You were there. You came over with some groups. I was talking about some of the work we were doing, and for some reason, we started getting back together again. Where the fuzziness for me is that Rockefeller meeting. Because you guys had done that, and done some meetings and so on, and maybe melded together.

RV: You were on sabbatical. You were on a post-doc.

BC: It had to be between Boston in '69 and the OSU '72 meeting. The Stroud was somewhere in that...

Jim Sedell: I thought it was after the OSU meeting.

KC: What was the IBP meeting? Was that the Oregon State one?

BC: Yeah.

RV: What year was that?

JS: That was '73. That was the spring of '73, because that was the first time I ever met Franklin. Because I thought the Rockefeller came after that one. And it was after that one that we decided we could pull something together. We wrote the little grant to the NSF to meet at Gull Lake...

KC: That's right. We were supported for that meeting at Gull Lake.

JS: Right. But I met Ken and got into streams because of Ken when I came to graduate school at Pittsburgh. And then I stayed when you went to Michigan State because I didn't want to go through all that orals and comps stuff again. Then, as I was getting ready to leave, I got a job with Jim Hall and Norm Anderson there at Oregon State on the IBP project, and so I needed to take a tour, and I went up to see Stuart Fisher, and they say "You've gotta see this Stroud." I'd heard lots of stuff about Stroud, but never been there. And so then I went and spent three or four days at your place. That's when I got

taken totally in. You I'd known, again, through papers, and I'd heard about you through the isotope stuff, and then through Dan Nelson.

BC: I don't remember where I first met him, but I can visualize it. He came down a row of seats in an auditorium, and introduced himself, but I can't remember where it was. (Everyone laughs) I remember that.

KC: It was at the meeting at Stroud that Charles Warren came, right?

JS: No. Charles Warren never left State, but when I got to Oregon State, it became real apparent that what they'd done for streams in the IBP was pretty parochial. It was fish, insects, and I wasn't going to make a living as a post-doc doing that, or being part of this project. So, Charles Warren was the big stream kahuna at Oregon State. He'd just published this book on water pollution in streams. And he had this, quote, theory. And I couldn't figure it out. I couldn't understand what he was trying to say, and so we suggested that he present what he was doing, and then we would bring a group from Michigan State, and group from Stroud, and the other place. And they all came, and we listened to Charles Warren, and weren't sure we understood him any better...

JS: So we had thought that in many ways it was a lot of mental masturbation, that there wasn't much there, and that the side conversations were far more interesting and important when we broke for coffee or had beers. That led to some thinking of ideas. I don't about...

BC: The Stroud was after the Oregon meeting?

KC: Right.

JS: We were out at the arboretum, and I think we went out to Berry Creek to look at that site, and it had kind of fallen in disrepair at that time.

BC: And they had that little chamber that you could sit down and look out and watch the stream go by.

VR (7:25): Robin, were you at this meeting?

RV: I was at the Berry Creek, and I remember Warren's talk and...Did he have the model at that time?

JS: Yeah. He had a family of curves. And if you took out a shotgun, and fired it at a wall he'd say "Ah! A family of curves! These are parallel curves!" But how did you decide where the point fit? Maybe it should have been on something else. I mean you couldn't ever...but you could draw a line through it.

RV: I think the major event that led my group at Stroud into the stream group here was probably the very fortunate event that we didn't get funding for IBP in our proposal. We

got an atomic absorption for five thousand dollars as a consolation prize, and we took the proposal to the Rockefeller foundation, and they were very very excited about it, and at the conclusion of our study, they required that we bring in about fifty scientists in this area and show the results of the study, and I think that's what brought in a really exciting group of people that I've interacted with for many years afterwards. You were, unfortunately, I think, at post over in England, was that '72? I remember Bob Wetzel was there, the Oakridge group was there. It was a very fortunate meeting for me...

JS: I don't think I was there... If indeed it was before the Warren meeting, then I certainly wasn't there. And then you had another meeting afterwards, where you brought Tom Dunn and other people in.

VR (9:33): Bert, were you at that meeting?

BC: No.

WM: But that's what I think really tied Robin into the whole operation, really, the context that you guys had and then the liaison you did between the two.

RV: I think that pointed out to me the importance of bringing groups of people in to look at projects, and tell storylines, get feedback, and hear what they're doing, on that scale of a group, of forty...twenty to forty people interacting in a narrower field than a national meeting.

VR (10:19): How did you organize it? Did you have, like, papers giving the overview and then just have general discussions or groups?

RV: Well, we essentially had our story of the White Clay Creek, and we presented maybe half a day of what we had found out in our studying. It was a capsule presentation. And then, some of the other groups gave little talks. It was very informal, though. The room wasn't even as large as this (laughs). There were forty people, I think! But, it was a lot of fun, and most certainly from that meeting, ideas flowed about how can we interact more often. Then, I think we did get together at other meetings.

KC: Then, we started meeting at the different sites once we got going. I remember that Idaho State was the first time we played Frisbee football I think...

BC: But, you're saying that was still before we started as the "continuum" thing.

KC: No. This was me actually planning, because I remember Brock was at that meeting. We were planning...some kind of planning meeting about...or maybe, you know, we had a lot of proposal writing meetings where we would get together and write proposals.

WM: So, the key ones were Gull Lake and Stroud.

BC: Okay, now we're starting what, in my chronology, is the first time THIS group actually got together and interacted.

KC: And we tried to write...the proposal that was rejected in...

BC: In '73...I guess I'm luckier because I had to get the work on this chronology for that talk I gave on it. So that was in '73, and I remember getting a call from Jim inviting me to it. We're going all to Gull Lake and we're going to try to get a group proposal. The NSF was receptive to something like this. But as I also recall, we did an awful lot of wheel spinning at that one, because what do we do at your site that compares to this, and mine in the desert, and so forth.

WM: And, of course, we were still sorting each other out. And there was no paradigm, nothing to really hang things on before that.

BC: And, as I recall, and as my notes say, all we really tried to decide to do was have another meeting at Stroud, and each of us would go back there and cover an area. Wayne and Robin were going to do invertebrates. Ken was going to do detritus. You were going to do geomorph, and I was going to do primary production. And we were all...we were still pretty, as I think, quite tunnel-visioned. We were still looking at these eight areas, same areas of expertise. What could we do to these flowing streams in different areas?

RV: One of the driving forces was the IBP money was drying up, and how were we going to get funding? Could we somehow join forces to come up with an idea of how to look at cross-biome comparisons. The difficulty was to come up with what parameters we were going to select to measure, and there was a lot of differences in concerns, because everybody had their favorite things to do.

BC: And even though we were supposed to come back and talk about the state of the art, those regions, it was still just that. It still wasn't really tied together.

WM: Well, the IBP brought us the framework of watershed level studies and nutrient cycling and energy flow, sort of, focuses. So I guess some of our orientation coming to the table was with that background and that experience. It was largely the driver of the IBP and the connections that I made through IBP that brought me into the...

JS: When I got back and found what I was as a post-doc at Oregon State was what I call fairly conventional fish or insect things. We turned that into much more an interaction with forest, using fishers of Bear Creek energy budget...carbon budget. And so that then allowed us to nest within a coniferous forest biome legitimately, and it just didn't look like disciplines were just attached to the coniferous forest biome. It became a real integral part of how that went about, but that also allowed us to start thinking about something else other than just communities of insects.

KC: You had some key players at Oregon State. You had Jerry Franklin, but you also had Fred Swanson and some other people who brought...all these places the geomorph input was pretty significant. I know the first time I met Fred, he said "Now let me get this straight, you really care about things that happen on an hourly or daily scale?" And I said "Yeah, yeah, we do." He said "If it's shorter than a hundred years, I can't really think about that." But I remember going out with you and Hank Froelick and measuring wood in streams. And that was a completely new kind of thing. So a lot was going, and I know that Franklin was involved heavily in IBP as was everybody there.

JS: Oh yeah. He and Dick Waring were Oregon State and the turf war was between Washington and Oregon State. The strategies were very different. Washington went with full professors and divied the funds out to full professors, and Oregon State purged the full professors and went with post-docs like Fred, myself and Frank Triska. Stan Gregory obviously emerged as a major find there.

WM: And at least in the history of the desert biome, the prima donnas were the dead ends. Basically, they wanted to do their own single focus kind of thing, and so going with post-docs was a really wise thing in IBP in the desert biome as with the younger professors, who also were more open to...

KC (16:57): A lot of people still say that IBP was a total waste of money, a failure. Franklin says "Well, no, because look at all the things that bloomed from that exercise. The meeting at Oregon State was the one that turned me around. That's the first time I had ever really gotten sort of the watershed perspective that you guys had, and the first time we met Franklin. Jerry's kind of a thread through this, including getting us funding when we'd been turned down by saying "No wait. This is a really good idea. We gotta get some money to these guys.

JS: Franklin never knew a good science story. I mean, he could spot a good science story. So his strength and still is to sit down with individuals, listen to what they've done and then weave a story from it. He could see that we were gathering the thread of a story here that he could weave with a terrestrial interface, whether it was different than what he'd been hearing through NSF meetings and things like that.

BC: But still, at that stage in our group, we were still looking. We were still spinning our wheels and having a hard time breaking out of what I thought was the conventional comparative – this, here and there. It wasn't until we went back to Stroud and invited a bunch of other folks to come back and share their thoughts with us that those papers (chuckles) as I remember, those papers really kind of fell by the wayside. I know I gave mine. I can remember giving it, but it seemed pretty dumb, quite frankly. It was: "Alright, here's the status of primary production now. So what?" I don't know if you guys ever even got up and gave yours, but by the time Robin really turned us on, that's where it really happened. I guess it was silly to give the rest of those, quite frankly.

KC: Yeah, it kind of lost its relevance.

BC: Yeah. Maybe I did it out of pure bullheadedness, but I was gonna give mine.
(everyone laughs)

KC: I've prepared my slides, dammit, now I'm gonna show it!

BC: That's right. And that was a pretty eye-opening thing when Robin really turned us on to that whole paradigm.

WM: As I remember that, I'm not sure how much prelude Robin gave, but let's just say he was droning on, and it broke up in the back. Perhaps Jim and Ken and several others were talking, and meanwhile I was paying intense attention to it (*everyone laughs*), but anyway, Jim jumps up and says "Wait a minute! What did you say? Go back to that again." And so Robin repeats his little storyline, and that was, to me, the spark – the moment where they said "We got something that we can hang a lot of ideas and a lot of this background work we've been doing. We've got a framework now when we put it together."

VR (19:50): Robin, can you remember at all, in the short version, what the storyline you told was?

RV: Well, we had started out looking at a third order stream, and putting all our efforts there. Halfway through the study – this was a three year Rockefeller study – it became quickly evident that upstream processes and downstream transport were a major factor in what we were seeing on this riffle/pool. A typical study that you start off with, and it was just that in order to understand the system, you had to link the upstream communities, upstream habitats, and what was going on on the flood plains, or the riparian zone in the stream. We just found too many mice in the stream, so much leaf litter. The transport system was very important. You have all these filter feeders. It was easy to begin to link all of this together when you're living on a stream five to seven days a week – many many hours studying something intensely. It was just a fortunate place to work. And then I think you see what was happening, so I want to tell these guys what I thought of how a stream was working in its nine hundred hectare basin, which is a very small patch of landscape that had been only twenty percent forested, and eighty percent in farming. It was very fortunate to be there at that certain time.

BC: The thing I remembered, and I may be wrong here, but this is what hangs in my mind, is you presented largely a physical story, and the opportunity came up for us to hang biological analogs on this. My recollection was that way, that it was a very physically oriented story, and all of a sudden, the opportunity came that "Gee!" we could tie biology into this.

RV: I didn't really start interacting with Luna until maybe a few years later.

BC: Well, that's the perception that I keep.

WM: Well, so help me with this. I thought part of the background, too, was that, whether it was a Rockefeller grant or whether it was another grant, you had an opportunity to go across the country looking at a variety of streams...maybe it was a sediment impact or a thermal impact...

RV: That was another very lucky...lucky...

WM: It was such a lucky thing. But...so the point was that in a very short period of time, a pressed period of time, he was able to look at a large number of streams.

RV: Yeah. The council on environmental equality wanted to evaluate the impact of five of the six programs from the soil conservation service, and the programs of the Army Corps on channelizing rivers. Forty-two streams were selected across the country, except in Mississippi. We weren't allowed to go near Mississippi (*everyone laughs*) but the chairman of the committee that deals with these sort of things was a Mississippian. But this gave me a very unique opportunity that select matched pairs of streams – a natural stream and a channelized stream – varying in size from a first order stream in South Dakota to the Kissimee to the King's River in California, and I saw how channelization affected stream communities, and how it affected sediment transport storage, what the meaning of a destruction of a ripple pool sequence, turning them into channels. That was a unique opportunity.

WM (24:41): Also, I'm surprised that you said that your interactions with Leopold were after this, because from the moment I met you, you were living and breathing geomorphology and thinking big scale sorts of things, and I remember explicitly when we did a little walkthrough at Michigan.

RV: There's a tape somewhere in USGS, I think it was in the late 1960's... '68 or '69 when the USGS came. The film team, much like this, and wanted a storyline of how Ruth Patrick and her group did stream surveys. They wanted it as a training program for USGS. I guess that's where I first met Luna, and then he would come out and teach a class at Penn for a couple of days, built that interaction.

BC: Bringing up Ruth's name, it also reminds me of a little sideline incident. We went out to have dinner that night, and Ruth was going to join us. As I recall – maybe correct me here – she wasn't wild about your idea for a while. You were a little reluctant to present some of these things, and then she sent word and said "I'm not going to be at the dinner" and if I remember, you relaxed quite a bit. (*everyone laughs*)

RV: Yeah I remember that. Ruth had a unique capacity to get money, and it was always good to be on the good side of Ruth.

KC: Came from money, and made lots of it.

VR (26:40): I was wondering...you mentioned Jerry Franklin and Fred Swanson and briefly Stan Gregory. Who were some of the other people that when you were still in these planning stages...who else would you guys remember?

KC: In fact Stu ended up being a reviewer of the program right through the whole thing.

JS: It turns out that people working on parts of the proposal in different places.

RV: From Stroud lab, it was Tom Bott that had been doing studies.

KC: Certainly Bob Peterson at Michigan State.

WM: What about Larson?

BC: He was at Gull Lake for that first meeting. Rick and Tom. These were the only guys that sent a team to the first meeting.

VR: Rick who? Rick Larson?

KC: And Wetzel was at Kellog at that time, so we had a lot of interaction with Wetzel.

WM: Pontification:

KC: Yeah. Pontification. But he was certainly tied up in geochemistry.

RV: Mike Kru...Klu...

WM: Mike Klugr.

RV: He did a lot. He changed themes a number of times, and he was at the Kellogg meeting.

KC: I really think that's how the microbiology got put in and the hypomyci fungi being the real keystone in the litter processing.

WM: So was Frank with you?

JS: Frank was with me, and he took part in the early discussions, but then once we got funded and a post-doc and a team, it became apparent that he was going to do his own thing and not mess with Bob...

KC: Was that BP money that was sampling all those...detritus in all those streams?

JS: He was doing that. He and I went to grad school together, and he was in the Army, and then when he got out of the Army, I hired him at Oregon State to be partnered with me. The other person at Oregon State was Dave McIntire. *(group all begins talking)*

WM: Jim Hall. In fact, Jim came to more of the meetings than either of the other two.

BC: Both of those guys were back at that Stroud, because I've got slides showing McIntire and Hall.

RV: I think Hall did a lot for us.

WM: Straight arrow as you kept referring to him. He kept you honest.

JS: Well, the way we worked at Oregon State and the way he did it was I would just roar out ahead, and he would...he had me on a choker collar. And so he would keep us honest, and ask for details. He asked tough questions – fill in the blanks...

BC: This was the question I was going to lead off the next...did you ever finish any of your ideas? (*everyone laughs*)

JS: My wife hired out people to do it for me.

RV: I think Norm Anderson also was near...he came to some meetings and I remember he came to Stroud and worked for a week or two on his type of work. I'm not sure why he didn't link up.

KC (29:57): Well, he was connected to the Oregon stuff all along at least peripherally.

JS: It's really interesting, the human dynamics, because when I came here, when I went to Oregon State, I was fortunate that as soon as I arrived, the people that were to supervise me and guide me intellectually all left on sabbatical. And the joke was that they came back, and they were working for me, because we totally transformed the study. We had these little bits and pieces of fish stuff, and insect, and nothing...and so we flipped it, and started doing a Stuart-Fisher kind of carbon budget that then we can attach the invertebrates to. We had a better story that fit with what we did. Norm was very very bent out of shape about it, essentially being thrown out. We stayed with him, and worked. I mean, he's still a very close friend now, and we all stayed together in meetings. So we worked hard at the group thing, even when really tough decisions were made. We talked a lot about that in the continuum – how we share credit, how we divvy up all kinds of things like that. So, people were concerned about how the dynamics of a group went, and how you try to keep a group together.

KC: What helped me was I took a sabbatical at Oregon State in '76. When I got there, I decided "I'm coming back here the first chance I get, because these guys..." That was my real first shot at a real multi-disciplinary thing. You guys had the whole kitchen sink...everybody. And nobody had the same ideas, and that was a time where...they were still getting paid to pull logs out of streams in Oregon in those days. That was a real culture shift that was science based. That's one of the most remarkable...and that's Forrest Service as well. It's a whole different philosophy of...something that was very

bad was now actually the most important thing going on. All the players were there to make that science based thing happen. That was a real exciting place to be. I remember showing up, being the only person at Oregon State that didn't have a beard.

JS: Now you're all that's left!

KC: I still got one!

WM: I don't want to keep returning to IBP, but that was my part...

JS: That's how we connected with the desert biome.

WM: It was my interaction with them, I eventually became the aquatic coordinator for the desert biome, so I was working with a lot of aquatic people, trying to pull some sort of scheme together, at the same time interacting with the terrestrial people. I got this interactive land-water thing from that orientation. With the storyline we really developed on our research on Deep Creek and so on, we got together and you kinda got "Whoa! We got something there! Let's talk." Then I was invited to go to one of these meetings, more or less out of the blue. I kinda felt "Gee, you guys have all bonded during this Rockefeller meeting" and I hadn't been there, and this was really a lucky thing for me to have made that connection.

KC: IBP was real important. It gets a bad rap a lot, and I don't understand that, because it may have facilitated a lot of this.

WM: The bad part, I think, came from this vision of modeling everything, and our belief that we just do it and hand it over to the modelers and they'll take care of it, and they couldn't do that. They lost track, and I think we got a lot of negative stuff from that. But if you look at the fundamental research and the interactions that were built and the broader scale of working, both at watershed level and beyond, but also in the team efforts, that's really where I cut my teeth.

JS: At the time, there was a world-wide split also, in that the international IBP was still into energy budget or trophics, and the Europeans were very concerned that they hadn't made the links, where even though the modelers were going rampant in the US, they would turn to nutrient cycling, carbon cycling, nitrogen...they were using different techniques to get at it, and so they were moving real fast.

WM: Any time we get a technology breakthrough, I think...you know now, with all this genomics stuff, the tool drives the science for a long time instead of the other way around. And I think that's what some of our work did, then we'd say "Flip out the modelers, they can work for us," maybe, but it's not working that way now, with the top-down direction with the modelers telling you how to perform.

JS: Ninety percent, at one time, of the NSF proposals in limnology had something to do with C14, and primary production. So that was in the early seventies, and then we started to flip around to detrital...

KC: We went to C13 so you could still do it.

WM: Yeah that's a great tool. What a powerful tool to have stable isotopes.

JS: But it was ninety percent of the money on ten percent of the problem.

KC: We were talking about it on the way over, the new genetic influx into ecology and fisheries and in DNA signature. It's another great case of "Here's a great tool, show me something to do with it." The problem is that the influx of the tool doesn't bring the philosophy and the interaction with it. Since molecular biologists are double recessive for eliminating ecology, you don't want to infect a system with that without some kind of way to make sure that ecology's still there. It's a great tool, but unless you've got the kinds of things we've been talking about...to me IBP brought a lot of people together. Some of the papers were good, and some of the papers were bad, and so on.

JS: But the interesting thing, there were interdisciplinary pockets that didn't get funded by IBP. You were part of the desert biome, right? That here, things were really going. Here, Stuart Fisher really never was, and I thought he broke some ground there at Bear Creek, there. It was just a matter of finding one another, you know?

RV: Well, it seems to me that many of the IBP projects could document and quantitate how a square meter was structured and functioned, and model that square meter, but there wasn't linkage with any other upslope, down slope areas. I think that science took a big step in looking at systems after IBP.

WM: It's true that the River Continuum idea that you presented is what brought about the linkages. What we were still talking about – the reach of a stream, or a riffle or whatever, and looking at that dynamic, and we did not make the connection, and we did not scale up to other sizes of streams.

RV: But the thing that was going on on the prairies, the Smokey Mountains, you knew what the...

JS: Respiration of a leaf was. *(everyone laughs)*

RV: Right.

WM: I think that's what you brought to the table, again, the fact that you'd looked at these streams across the nation, and you saw the dynamics operating basically similar in each of those, and so you could see a paradigm or a model there that tied those together. Many of the others of us were still looking at our own little world. The desert biome versus coniferous forest, or whatever.

BC: That led, to me, to what was the most exciting, I think, week I ever spent with you guys. That was when we went back to Stroud, or no, it was Gull Lake, and took over George Lauf's house to write the proposal. That was really something. Remember hiring those women...house...what were they? Those typists. Those poor gals. Everybody was in a room, writing a piece of this and "What are we going to measure?"

WM: What year was that?

BC: '74. July '74.

KC: And Lauf's mother was like your mother, very sharp, so she was wandering around this group, and George kept trying to explain to her what was happening. She said "What are all these people doing in my house?"

BC: That, to me, I remember, was something. There was more generation, and everybody working like this, and typing girls who didn't know the language...

VR (39:16): Before we go into that, can we go back to that meeting where Jim said "Robin, repeat that" and lead from that into the meeting you're talking about.

BC: That's kind of where we are. Robin presented this idea, we got all excited about it, we said "We gotta go back home and think in each of our little areas that we were most interested in, of how it fit into this into a downstream-upstream linkage sort of thing." And then we met back there to write the proposal and tried to put all of this together, because we were still looking at "What are we going to measure for the primary production?" because that led to the workshop on primary production, where we were still grappling about "Are we going to do upstream-downstream oxygen chambers?" all this stuff.

JS: We went right to the details. (*everyone laughs*)

RV: Everyone had their favorite way of doing it.

BC: So that's what led. From that meeting, we all went home and started thinking linkage and upstream-downstream.

WM: Did we just hear from Robin on the last hour of the last day and then we just went home?

BC: It wasn't much more than that, as I remember.

RV: We spent a whole day on it. (*everyone talks*)

JS: The details bore me, but when you tell a story about it, yeah! I can see a story! I could talk to a taxi cab driver, and start to make some sense about what we were trying to

do after we saw that, and so that was important, because I really do believe that at our best, we tell stories, and so he told a story, and it was a compelling story, and then it was a matter of how you shape it. But it was the only story we'd heard. We'd been together a couple of times, and all of a sudden, he launched it.

RV: I was saving it for another year... (*everyone laughs*)

JS: So we kept pushing to squeeze it out of him.

RV: Luna would be the best person to talk to about how it came out.

WM: Why do you say that?

RV: Um...I don't know (*everyone laughs*)

BC: Anyway, it led to that next Gull Lake thing, where the proposal was actually written, what we were going to measure, where we were going to measure it, but I remember for another reason that these other guys probably won't, but that's when we came up with "We're going to do a continuum from low order to high order in the biomes," and that's where I didn't fit in. Because the Columbia wasn't going to be workable, and the little desert streams weren't going to be workable, because they didn't have a continuum.

VR (42:20): So this is the Stroud meeting you're talking about?

BC: Yeah.

WM: Alright, Bert, come off of it. We'll take care of you. (*everyone laughs*)

BC: You guys could have dumped me there, probably, but instead I worked with Wayne and was given some overall responsibilities for some things.

WM: That's really where that triple A S meeting was important, because that's where we met, and we really hit it off for some reason. I dunno, Montana – Idaho kind of connection, or whatever. And then through the IBP, we further bonded, and then we've been friends all the rest of our lives.

VR: You remember Louis Tralta making the statement "You need somebody to hold one end of the Seine."

JS: Well, we can get into that later, but just group karma, group ability to hang together needs a whole different kinds of buffers at different times, and other individuals when we feel boxed in a corner, you could talk to, it can kind of be intermediaries. Bert, aside from the technical expertise, provided a lot of that buffering and interpretation and things that sort of was important.

WM: Stability and carry-through, just a whole lot of things that were really important.

KC: George Lauf should be thanked for turning his house over, though. He said “You guys have got to get away. You’ve got to get out of this lab. You’ve got to have some place to go. I’ll just give you my house. And I’ll just go stay with someone else...but my mom still has to be there. Take care of her every now and then.” That was great.

WM: And I don’t have a good memory of that.

KC: She was up on a ladder, don’t you remember?

WM: And that was ’74...I remember the castle, being up in the castle. That was different from Lauf’s house, right?

KC: No. The one with the skylight and the shower?

WM: Yeah.

KC: Yeah, that’s his home.

WM: Oh. Okay. Alright. It’s a beautiful place.

KC: I remember his eighty-nine year old mother, up on a stepladder trying to change a bulb while we were having this meeting. It was a good setting.

BC: That was a wild session. Everyone trying to put it together.

KC: Felt like the Washington Post. Going to press. We were always on a deadline for that thing.

RV: We didn’t have some of the pieces put together, because Jim and I stayed up until five in the morning at some point.

JS: We redid one, got it in. We went back to Stroud, because you guys were all there, and we hired another secretary, and I became the go-between, and that was organized. We delivered it, and then we waited. Finally we got this call from Franklin, that it was on the bubble, but really rejected. Franklin liked the idea, he really liked the storyline, could we come in and talk to him? We talked to him, and I think that created another Stroud deadline, because we could always grab a train at six in the morning to dump it off, hand deliver it.

WM: Somebody hand carried it down there.

JS: Oh yeah.

KC: Franklin pulled it out of the ashes, and said "This is an important idea."

WM: It was director's prerogative. So he overrode, basically, the panel.

KC: Now, was the second cycle when we had to go to the board because it was over a million dollars? I remember the big thing about renewal being...the renewal was over a million.

JS: There was a track record. But those were the days when the NSF had a little more discretion. I mean, Callahan was there and he was an early skeptic, but then became a huge advocate, and then Mel Dyer followed Jerry Franklin, and the Amazon work that Jeff Richey and that group came on, and that again was one in which people like Bill Lewis just hammered it, because it was a big picture story, and it only had a couple of preliminary data points for the largest river in the world. They were drawing strange curves, and he had a meltdown and Dyer said "We need to take a chance on some of this to get moving in big science." So NSF had directors at that time that were willing to take a little more risk than what they're allowed to legally now, or whatnot.

WM: Probably, the directors now would take the risk if they could. It's just that they can't anymore. Mt. St. Helens might have been one of those hand-slapping incidents where some risks were taken, and it just bombed. What a waste. We're not ever going to do that again without a full proposal and all this stuff laid out, and so on. I'm not sure that everyone would agree with that, but...

JS: Well that's another one where they did, and they didn't, because there were a number of NSF proposals that stemmed out of St. Helens that did persist for several years, but the group never coalesced into a big proposal. It was either colonization of insects or plants, or what happened in the ash zone, or there was little lake surveys.

KC: Who signed the paper that got the grass seed thrown out?

JS: Well that was NRCS. That was a political decision to show everyone that we were doing something for the world.

KC: That's what fed the aquatic insects. And the elk. Grass seed. In a system, it would never support anything.

VR (48:11): So, these two meetings...you went to Gull Lake. Before you went to Gull Lake, you sort of worked out your methods that you brought to Gull Lake. You spent five days working on the proposal at Gull Lake, and then how long a break before you went back and met at Stroud to finish the proposal up?

KC: It was pretty quick, wasn't it?

JS: Yeah. It was only a few days that we did it. Then we all went back, got rest, rethought it, and then we came back to Stroud to finish it off.

WM: Must have been the dead of winter, because we have photographs of people in heavy coats and such. And then we left Kellogg in cold. But now, this is the second Gull Lake meeting, right? When we talked about detail and methods and so on?

KC: When we were at Laufs was in July.

WM: Yeah. So I just wanted to separate for Vince that there are two different Gull Lake contacts here. It's the second one we're talking about.

VR: Which one did you stay at Lauf's house?

Everyone: The second one.

BC: That's when Ken and Wayne wore their Omar the Tentmaker pants. Awful looking pants.

WM: Bellbottoms and plaid. Plaid! That's right...

KC: Yeah. We were class, no question.

BC: I shouldn't talk, because there's a picture at the PR workshop, and I've got a pair of plaid pants on.

VR: Well, this is sort of a remarkable story about Franklin overriding it, too.

JS: You need to talk to Jerry on his story on that. There was no question, because, again, the frustration they had was that there were no storylines coming out. It was more same-old same-old, only with better technology, or to get more precision on the measurement, but not... a concept change, and how we were looking at the world differently. And that's what Jerry saw in the continuum. We were moving a different way of looking at the world. That was worth taking a chance to fund, and he was also committed to this cross-biome comparisons. He's always big on that.

WM: So, he played two key roles. One was the workshop money. However that was laundered, or brought to bear...

JS: Well, we wrote a proposal, and submitted it from Oregon State.

WM: But you must have had some encouragement to do so, right?

JS: Yeah, from you guys.

WM: You think so? I thought you were on the inside track with Jerry.

JS: Well, we vetted it with him, and he said he'd entertain a proposal to get together to

see if we had anything. And that was key. So, we wrote up a little proposal, said what we were going to do...

KC: At that was what funded the Gull Lake stuff.

WM: And the Stroud meeting.

RV: We all went down to NSF for a conference meeting with them. Jerry called about the reviews...

JS: We did two. One to go down and talk to them, privately, so that he could get a feeling of whether it was worth our while to continue, and then the second one was when we got the reviews back. He then told us what he needed if he was going to try to push it forward anyway.

VR (51:34): There wasn't actually a re-submittal?

JS: No.

VR: And Jerry Franklin was the program manager of ecosystems?

JS: Yep.

BC: I think it might be also for the record here, to point out that at that Gull Lake thing, not only did we write the proposal, but an integral part of that was determining the responsibilities of the PIs at the four biomes and that we were going to hire a post-doc to coordinate this whole thing.

VR: Just the five of you were at that meeting at Gull Lake?

WM: No. Klug was there. Bott was there. I remember interacting quite a bit with Tom in writing parts of it, and so I think maybe he was there.

KC: Peterson was in Sweden.

WM: Yeah. On one of these times, though, that's where I met him. You said "I've got this guy I want you to meet," and so one of those two meetings...

BC: It must have been the second one.

RV: Well, Bob was at Pittsburgh too, wasn't he? Because he started his job some time after you did.

JS: Right, but he followed Ken to Michigan State. He'd been just there a year.

WM: So he was out there, and you had gotten a big detritus study going. So he was out there helping you with the lab streams as well as Dusty Creek.

Group Meeting 2

VR:...start to appear, and you all started to use it. You've mentioned several meetings – the one that Robin gave the inspirational talk at. Did you use the continuum?

RV: Yeah,
streams...

VR: Did you coin that term?

RV: I don't know if I coined it. Continuum has been a word used for a long time.

VR: I know the continuity of rivers was discussed by geomorphologists.

WM: I think that Robin used it that first time that we heard his ideas presented. Sort of a continuum of changes.

KC: Because we were all segment people until then.

JS: You guys were into zones.

KC: Yes. Yes.

VR: Typologies.

WM: Yeah. And you know, even James has written recently, and he still hasn't got it. This idea of continuum moved in transition along a gradient is the important thing, rather than the breaks, and the whole European model – it just takes away this idea of transport, for example, being important in moving all the way down the system

KC: I remember thinking at this meeting you talked about, when you started wringing your hands, the idea of the downstream systems are living on the inefficiencies of the upstream systems, "Now there's something nobody's ever mentioned."

RV: And that's where the pollution aspect comes in. Looking at so many streams that were polluted and didn't have the structure of the communities that utilize the energy, as streams become progressively impacted, they lose species and they lose energy downstream.

KC: I remember the other thing from that. I'm not sure if it was that meeting or the one in Idaho, that all these bugs were really evolved to kind of play the game. Play the best stock. You can't predict what the hell is going to happen. Throughout the day it could be high or low, but they had put their money on the best bet.

BC: They made the best they could do with what they were getting, and asked the rest of us for...

KC: That's why they got fooled so badly by all of those dam reversals of flow and release of water during the dry season, holding it back during the wet season. That's how Florida got totally screwed up. You hold the water back when it normally came down, and release it when it was normally not there. No wonder they're confused.

VR (2:50): How did you guys communicate pre-email? Did you ever phone call, or was there fax? I mean...you telephoned each other a lot.

RV: No fax. Fax wasn't around.

KC: Ton of phone calls.

WM: And then, through the mail, any document that needed to be transported.

KC: I mean, all of this would've been spam now.

WM: Extremely low tech. That was why on these very short time frames, the Stroud location close to DC was important.

KC: You just hopped the train, and you hit the funder.

VR (3:40): Let's actually talk about the funding for a minute, and the grant. When you were proposing this, there was both a research component, and a data gathering component. Was the data gathering kind of the preparation for the proposal, kind of like when you had called me, that I mentioned, about Minckley's study? Do you remember how that worked?

KC: You mean when we were trying to get the story together before we...? Yeah. We were all going to present some little compartment of this. We were still segmented.

BC: Yeah, I don't have a big recollection of that incident, to tell you the truth. I just know we were all scrambling for little pieces.

WM: We got the inter-biome comparisons thing, right? We made phone calls and got together each day we could.

BC: name called us a phone call, because I remember that I called to get the stuff, and it was Minkley's. I don't even remember that, but I know we must've been doing a lot of this.

KC: Well, we had methods workshops.

JS: Those came before, but beforehand, there was this series of – starting at Triple A S and the Rockefeller and then Oregon State and then at Gull Lake – I mean, we essentially got that to get together to explore cross-biome on streams. That was an NSF funded

proposal for about 25K. That was really used to move us around, I think move to that final Stroud meeting. That was when we got to explore whether we had something worthwhile to put together. Really, the outcome of that was the big proposal.

KC: That was not methods oriented at all.

JS: No it wasn't

KC: So we had to backfill with all the methods after that. We had a lot of discussion in workshops and stuff to try to figure out what we were going to measure.

BC: We had a PR workshop, and a detritus workshop, both at Stroud. These came after the Pocatello meeting in '75.

KC: When was it we got Callahan into the river with us?

BC: I got that as...that's down the road here. It was the detritus workshop – December '75. That was preceded by the PR in August '75 when, man, we all descended on that Stroud meadow with boxes and...

RV: Chambers. We had respiration chambers and...

KC: Remember how we all had bottles, and you said "Everybody sample"...

WM: No. That's the detritus one. That's the second one. That was a review for our second proposal, right?

JS: And Callahan was there.

WM: Yeah. And Risser, wasn't it?

BC: No. It couldn't have been, because, from the stuff I'm working from, in December '75, we had to standardize, and decide what we were going to use for metabolism.

VR (6:38): This was after the grant was funded?

BC: Yeah. And we had the in-stream boxes, we had the Stroud ones that were in the channel outside, brought it upstream, downstream, oxygenated, and I ended up checking out the pH.

KC: Donna King, was Donna King at that one?

BC: Yeah, Donna King was there, and that was in August of '75, and then we added them up, and decided that the in-stream boxes were the way we were gonna go. In December that year, we met to look at detritus. We tried the nested nets, the boxes, the bomb, literally bigger than legal, and it was at that time that we did the great carbon

sweeping rocks thing, and Callahan was there. He got the only pair of leaky boots in the bunch.

WM: Huh. Why would Blair and Callahan have come?

BC: I have no idea.

WM: We up for review, and we were stranded..."What are we going to do with all this time? Let's go on out, and try this cooling experiment." The two had to come together.

RV: Because we went out to our site on the King Ranch for that big sweep.

KC: Oh, that's where it was. Yeah. Right.

RV: That was after our first year of study.

BC: Well, I can show you the slides that show us trying the nested boxes, the bomb thing, all these. And they're days at the same time as the big carbon experiment.

KC: We might have tested them again.

WM: I don't remember that, but I do remember that they came as part of a review, and we utilized that opportunity to go out into the field, in part because we didn't know how we were going to fill two days with presenting them with one idea...

KC: But every time we got together, Brock would have some gizmo for something.

BC: I dunno. Maybe...

WM: Bob Peterson was with us at that time, too. So he had been hired at Oregon and functioning for some time.

RV: That was probably after our first year of sampling.

BC: We had a workshop to test the devices and what we agreed on after that, was that it didn't matter which of the methods we used as long as we all used the same mesh sizes. We didn't care whether you pumped it through, whether you put the net's anchor, the only key was whether you could know how much water went through, and we used the same mesh sizes.

KC: The purposes of the broom experiments was to see whether, in fact, you could sample the thalweg at one place on the stream. Because we had everybody across the stream with their bottles, and we filtered it all. We waved the filters around, and you could see the...

RV: That might have been in preparation for the Salmon River study.

JS: Yeah, I think it was.

RV: Not the initial selection of methods.

BC: In that case, I've got a mistake here, because I thought...

WM: Well that's why we pushed it a little, because we thought you were fallible, and...
(*everybody laughs*)

VR (9:58): Had you guys started any writing that led to the 1980 paper at this point? Was anything written down?

JS: It was primarily from the original proposal.

VR: So the original proposal was what led to the 1980 paper?

KC: That's the short answer to that question.

JS: I mean it really then got tailored for pub, because I mean in my recollection, the storyline was there, and you could understand the storyline from the proposal, but for a journal article, it really had to be edited and picked up.

VR (11:22): So, let's hear the story of the 1980 paper, the classic, three times more cited than the next paper in aquatic ecology.

WM: I think that we have on tape already, perhaps. A lot of this was from the proposal itself. To me, an important requirement of the funding had been that this would be put out for raw peer review. By requiring publication of it before our renewal would come about. So we had this, I think, healthy pressure on us. And we've seen this in other of the NSF's actions subsequently, where early on, the ideas have to be made public in a published form. We were coming up on a deadline to get that out. So, we put together that final document that was initially submitted to American Naturalist. We got a relatively quick response back, because the editor refused to send it out for review, or to have other people consider it. He unilaterally said that it was rejected and being returned, because there was no testable hypothesis there. Soon after that then, almost instantaneously, because we thought the manuscript was in good shape, we flipped it over and submitted it to the Canadian Journal of Fisheries and Aquatic Sciences. They were willing to review it, first of all, they sent it out for review, and the reviews were constructive, encouraging, basically it should be published, but with the normal modifications, and so on. So, it was honed and resubmitted, and I think we could claim that it was at least in press by the time our deadline was up, if not already published.

JS (13:15): There's no question that getting the paper out was important for renewal, because NSF had gone out on a limb to fund it, because the initial reviews for it – it just

came out of the blue for streams – and it didn't get the highest reviews. Some people really liked it, but overall, the average was not a fundable score.

WM: It's certainly straight from Jerry's hand.

JS: Right. And once the paper came out, he had something to justify why he would go out on a limb to get it going.

WM: And then to go back in front of the panel. "Hey look, I took a chance, but here it is, it's out there now, and we can be critiqued by our peers much more profitably."

KC: So then we hired a post-doc who would...once we got it started, we hired a post-doc who really got to work. The idea was that everybody goes to their own site, and does all these techniques, and then the next cycle, we all go to the same place. I can remember the picture in the Salmon. We had how many? Fifteen PR chambers out there. You look out there, and it's just this ocean of chambers with batteries and wires and everything.

VR (14:40): Is that the second grant?

WM: Yeah. I did do a fieldtrip out there. I recall that, where we were standing out on the banks, checking out the river systems, and Hall, I know, was along on that trip.

RV: One thing that might be interjected at this time is the putting together of a data recording sheet for the first year of study. In each biome, the data sheet had to be completed so that there was a number of transports in each category, that was a number for primary production, there was a number for the detrital leaf category. There was some initial opposition to having a uniform data sheet for all three. Keating, who was an important confidante of Bob Peterson's put this sheet together, and then the data could be scanned optimally. Everybody had to have the data sheet set up, and this was very important.

BC: This was Bob's big dealing...

RV: Data management.

BC: And also, it might be good to have on the record that Bob spent part time at Stroud. He stayed there for how long? I think he was at Pocatello for a...I forget his time was divided.

WM: I think he was at least at Stroud for the first year.

RV: Yeah. Maybe even the first year and a half.

BC: And then he went to Pocatello for the last part...

WM: Especially when we were setting up to do the big river project.

VR (16:45): One of the things that after the 1980 paper came out, there was immediate embracing of the idea by the vast number of researchers, but there were two papers that came out, one of which had a really negative tone, by Winterbourn and Rounick – maybe I've got the authors reversed – about New Zealand streams being different. Did you think there was anything constructive in that paper?

WM: I have a couple things. I won't answer that question directly, but the first thing is that it was published in what was a relatively obscure place, so it didn't surface right away. That was sort of....there was some lag time before that really came to fore, although I thought Rounick had been a thorn in people's side from even Oregon days.

JS: Well, I wouldn't call him a thorn. He came out and did his undergraduate thesis for Hampshire College. He and Nancy Grimm were both Hampshire graduates. And so, they came out and did their senior project at Oregon State, and so we knew a lot about Jim Rounick, and Jim Rounick was a tremendous verbal communicator, and couldn't write for diddly, but he had an amazing ability to go and keep talking to someone until he got...like Dave McIntire and others. And then he went with Winterborne. Winterborne really taught him how to write, and then he was all excited about it. He was always challenging you, and that was a plus for us.

KC: He was infected by Dave Rouster while he was at Oregon. They gave him the Dave Rouster injection. He and Nancy both. I'm not sure that they ever really recovered, to tell you the truth.

WM: Well, it seems to me he also had something on about functional feeding groups. He had a hatred for them. He could not think rationally on them. And to me, that paper that he and Winterborne wrote was more fueled by that dislike and disaffection with it than it was anything about the River Continuum. And I think, my personal point of view is, knowing now more about those streams, they're sheet disclimaxes, so in a way, they really are different. And in another way, the water still runs downstream, and if a leaf were there, it would get chewed up by the detritivores.

KC: They also had a meeting in New Zealand. I went in the north island, and I went to the only remaining old grove forest that's in the north island, they have a park, and it's full of everything you'd expect to find in a forested stream. When the Maoris came in, and cut the damn forest down, killed all the flightless birds, and now they're highly revered for that. They changed the landscape of the north island. The south island is a whole different thing. That is a major geomorphic...completely resorted island every year. They are different, but the invertebrate story that's different in the north island was they took the story out early on, but if you go to Curry Park, it's just classic. I mean, it looks just like...all the places in Australia look exactly like that too. I think they're different. No question they're different. But I think they forgot to mention some of the reasons why they're different.

RV: The deforestation in Australia, and the impact of the sheep there is the same. Then there's the meadows, the streams, the reforestation programs in Australia.

VR (20:32): Although Lake did not object to it. Lake's response was not a negative one to the continuum. He wrote a response, I think a response to Winterborne is what it was.

WM: I think Sam had a bigger perspective, and didn't get locked into that. And Winterborne, first of all, by printing it where they did, they thought they could get some mileage out of it, without it being as high profile as it became. I'm having trouble remembering this, but I don't know that it ever bothered us enough that we thought it had to be answered.

JS: Oh, no. We invited him back to Oregon State for a seminar. He came back, spent a couple days, gave a talk, Stan and I were there. I don't know if you were still there.

KC: I was there. Yeah.

JS: And so, he was very excited about what he was doing, and we couldn't get excited about it. You know? "Great job, Jim." It wasn't like it moved us to want to change it, or that he had an alternative, a concept that fit something better. I mean, okay, there's an anomaly here.

KC: Sam Lake and Ian Campbell, and some of the others put a workshop together at Threadbowl in the Snowy Mountains. After that, Ian organized a camping trip with Winterborne and a bunch of us. And we went to a lot of streams, and so it was pretty clear that Winterborne had not looked at a lot of streams other than New Zealand, because what we were shown in Australia was some pretty pristine stuff that's still left, and it all fit this. You know, you've got to look at the whole basin perspective. He's a really nice guy. On that trip, everybody'd be talking about this, and I think that if you talked to Winterbourne today, you'd get a whole different story than you get early on. I don't know what role Rounick played. Was he the one that injected the story into that?

JS: I think so. I think he pushed it. And again, Jim's a perfect straw man to look for exceptions or whatnot, and so he was very excited about what he'd found. Probably, like you say, much more so than Winterborne. Although, we had Winterborne out too, because Norm had spent a lot of time with him, but Winterborne didn't have the same passion of the paper that Rounick did.

VR (23:22): There's another paper by Statzler and Higler that took a different point of view. Many of their comments were language. I think it was a different interpretation of...a literal interpretation of words. And then, of course, then they had the hydraulic overlay that they added. I know that he went to Oregon State. What was your feeling about that paper?

WM: I was actually asked...that really stimulated the paper that we followed with, the modifications, the clarifications it really what it was.

VR: And where did that paper appear?

WM: That's in Canadian's journal as well. Because it was appropriate and I think at the time that it was such that that would be the follow-up. That would be where we would go with it. Having been told that we didn't have a hypothesis to test anyway, we wouldn't go back to American Naturalist. We were asked to comment, to write a rebuttal. I think the editors contacted us, and asked us if we wanted to write a rebuttal. And basically, we had a lot of iron in the fire, and were pretty busy, and I didn't want to spend the rest of my life being a defender of the River Continuum concept. I thought it was out there, and people could interpret it. I also thought strongly that there were some language problems at the time. I think we were scheduled, maybe you initially and then we decided to do a joint presentation at a NABS meeting in...Duluth? Was it in Duluth or up in that...no, it wasn't there.

KC: Lacrosse.

WM: Yeah. Lacrosse. So, I basically, Ken was tied up with other things, and I gave that presentation, and it was the core of what we went to Canadian Journal with, but it was designed to and answer those valid criticisms that Statzner and Higler had. So that was to be our answer, without having to go to every damn brush fire and trying to put it out. We tried to sort of say "This is our thinking now, this is our update of it, and this addresses some of those issues, maybe the most important ones that were raised in that paper."

VR (25:38): This is where the scale would slide? That was a great addition.

JS: So, was Webster one of the original reviewers of the proposal, or the paper? Because he signed his review, and he was very critical. I remember we brought him out to Oregon State, and I think we were there to essentially get...We brought him on a review to get his ideas, and man he didn't change. He was just locked in as a critic. Again, it's one of these things, and Statzner the same thing. I don't know if Statzner felt as deep a philosophical difference with continuum ideas as Webster did, but Webster became a big proponent and essentially...there was an amazing ability among the stream community, and it may still be there, not to get personal. So you could just be terribly vicious in the intellectual discussion, then we'd break and go for a beer, and we talk about things and find in common. Yeah, we could be harsh, but it never broke away, and we continued to use Webster as a sounding board.

RV: He went on the site review.

JS: That's right. That was what it was.

KC: It was Fisher and Webster who came.

VR (27:15): And Jim, I remember you and Peterson went and visited Statzner in Karlsruhe. You said the same thing, that you had these long, long discussions, but then you drank wine all night.

JS: Oh yeah. Right. I mean, he could disagree... Well, I think he could sort the person from the ideas, and he didn't have to personalize the idea for it.

WM: I think it was at Calgary where I got on the bus, and Statzner comes up from the back and sits down. "I've been wanting to talk to you about this." And he started wagging away at me. When we got off the bus, we started drinking beer, and next thing I know, he's on a bull.

VR: I think that's the great thing about field biology. So, can we talk a little bit more about the research? Robin had just mentioned this data sheet that enabled you to do that. What are some of the other innovative things that you think enabled the research.

KC: One thing about that that was so neat was that this was a minimum set of things that everybody had to enter. But you were allowed to do anything else you wanted, that you were willing to support, and so there was a lot of insularly stuff that came in, but everybody had the same set. Otherwise, you could never have made a good bio paper.

RV: It forced people to be on time, do the work that had to be done.

WM: Our side-story was we developed the peanut butter and cracker idea, where we would put peanut butter on the leaves, and try to run and... *(everyone laughs)*

KC: I've got a picture of one of us, maybe me and Ellsworth...

WM: That's the turnaround. It's only fair that he could help you with that.

VR (29:20): How about Bob Peterson? Can we talk a little about him, because he's the one person that we can't hear his story. What do you think his contribution was?

RV: Oh, he was critical. It was a critical position, and he had enough muscle to get his job done, and that was to make sure we got our job done, and collected the data. We did have, Cindy Staub was the data manager at Stroud, and she was very good too at putting together the data slips.

JS: But the analysis, just the analysis and all of this was Peterson straight.

WM: On a couple of these, Bob came with the data. It was all laid out, he could manipulate it, he could get the means and variances out of it and so on, we could talk graphing and so on and so forth. He was really critical. And he had the ability to push. Sometimes too much, and he pissed some people off.

JS: Yeah he did!

WM: But he would get it.

KC: Remember the PR workshop where it was a hundred and forty and humid at Straud, and these guys are out all day, it's a twenty-four hour study and everyone's got their own devices, and I remember it was the middle of the day, we were sitting in the stream, just about dead from all of this work, and Donna says "Bob, go get us some ice cream." He said "How can I keep it cold?" And she said "Put it next to your heart." (*everyone laughs*)

BC: I'll throw this in too. When we were up at the ranch, after one of these sessions – I don't know if it was the one with the divers versus the bug-pickers, but I remember sitting on his porch...

VR: This was at Wayne's ranch?

JS: No. This was at a place near Twin Peaks, where we were doing a...

WM: When we were working on the Salmon.

BC: And he was sitting there, and he said "Cush, I tried." That's all he said. "I tried."

JS: He had a tough job. A real tough job.

BC: We're talking about McCullough and Hawkins and Naiman and trying to keep those in different camps. It was really tough.

WM: I really bonded with Bob when he came out to work with us in getting it set up. I loved the guy. I think he was one of the best people I ever worked with. Then when we had to do the writing, he would be there. He would come and spend a week, or whatever it was, have a lot of stuff together, we'd bang out ideas, he'd take a section, I'd take a section, and he was able to pull a lot of information for us.

VR (32:11): Was the colonization paper that you and he did, was that part of the continuum or was that something you did extra?

WM: No, totally different.

VR: That's a great paper. It really is.

WM: It was a lot of fun to do. And that's the nice thing about Bob. He could spin off his own set of really sparky ideas, and get excited, and just take it and run with it. Well, that was one where I'd actually been asked to do this presentation, to write this paper in honor of T.T. Macan in a series of papers that were being presented. I sort of had some of these ideas, I had some data, but it didn't go far enough. Bob and I started talking – I think I might have been on sabbatical leave in Pennsylvania then – Bob came in, we

spent a week walking lanes, talking about it, just sort of banging out ideas, and he brought a lot to that paper. A lot of different ideas.

KC: He was good. I remember when he was at Kellogg, he would come in in the morning, and he'd say "You know what we need to do..." and I knew that meant another six months, but I knew that he'd complete it. His thesis is still one of the best insect ecology things – it's not published – that I've ever seen.

WM: On *Nigronia*?

KC: *Nigronia*. It's a classic piece of work, it's just never been published.

WM: We called him seventy percent Bob. He'd take a project to seventy percent, and then he'd be diverted for one reason or another.

JS: I can relate to that.

KC: As Robin said, he was really critical I think, because you had guys like George Spengler who were really good at filling in data sheets, but without keeping all of that stuff together, it would have been just a mess.

WM: And his computer expertise at that time was really good. We had large data management, and then stats analysis. Those were just sort of incomprehensible to a lot of us.

KC: Yeah, I know. He just loved his Macintosh.

VR (34:22): One last question about him. Why did he go back to Sweden? Why didn't he stay in the US?

KC: He went, took a post-doc at Umeo?

VR: No, no. After the continuum.

KC: Well, his wife was Swedish, and he met her at Gimeo, and brought her over to the States, and I think his family was the top goal in his life, so he would do whatever it took to do.

BC: You know, I may be wrong, but a recollection to me was that he had a good chance at a job there, better than anything he had here. He had these things, and this offer was better than anything he had. I don't know whether I'm mistaken here, but...

JS: But the issue he had in Sweden was learning Swedish. When he talked to Norwegians and the others, the thing that was holding him from getting a permanent position there was a language thing. Seminars were always in English, and I'm sure by

the time he had spent ten years there, he could understand Swedish, but he wouldn't speak it.

KC: He spoke it in the market, but not at the conferences.

VR (35:55): When we go on from Bob, is there anybody else at Stroud or OSU that you feel really should be singled out from there work, in helping get this continuum to work.

RV: Tom Bott.

JS: Of course, Fred Swanson, and Stan on the primary production.

WM: In the formative stages, not so much Stan. But in the writing and the thinking, Tom had a really big role, and probably Swanson did too.

JS: I know Klug did.

VR: This is on the subsequent papers after 1980?

WM: Well, yeah. Tom...

JS: Tom was there for the original.

WM: Tom provided the inter-biome comparisons data to go, and we had long discussions about it, he did the writing on that aspect of it, so apart from where he took to leave, he was a great team player.

KC: Oh, he sure was.

BC: I still don't want to put Stan too far in the background, because he and Tom were the real drivers at the PR workshop, really thrashing out the data at the end. So, Stan was close then.

KC: Well, you can't forget Jim Brock. He was there all the time. There's nobody in the world that can muster the field effort that Jim Brock had. Three days of Jim Brock would kill all of us.

BC: I think we should document, that he worked like that, and then crashed. We've got a series of slides of him crashed in a sleeping bag in various locations.

KC: He came to Florida, and did this chamber stuff in the Kissimee. He brought his mother and she worked. She would change the chambers in the middle of the night. When you're around Brock, you just get sucked in. You work your butt off.

VR (38:19): How about input from other people? Was Fisher still giving you input and

ideas. You mentioned Webster before. Non-OSU or non-Stroud people. Other people. Were they affecting the way the program was going too?

JS: They were on our sounding boards, and while I don't know that we changed what we were doing because of interaction with them, necessarily, later on, they were good gentle critics. Jack Webster was a very harsh critic, but Stuart was a lot more gentle. He could see the storyline, he wanted it developed, and he would get on us with criticism. Again, there's that openness among stream people that when you did go to Fisher's shop, you came away with something. He didn't hold back a lot of things. He didn't think you were going to steal ideas or something. So there was an openness, an exchange that was about. Certainly Fisher and Webster were two that kinda stayed in and pecked away at us, and kept us honest.

WM: Most of these in general, they were ecosystem level people. There weren't that many folks out there with that, and that's still true. The core of the ecosystem research in streams is where these folks are from, you know? So I think that's really critical. Webster was an IBP person as well. He had that kind of a background and orientation, and he gave us access to that whole group of folks. The other person I remember is Wallace. We spun off a lot of things from Bruce, and I don't know whether he was a reviewer or critic in that regard, but we had a lot of interaction that wasn't formalized.

KC: He was a big one.

RV: He was thoroughly enjoyed.

KC: Remember he dropped a boulder on his foot when the topless females came around in a boat? He was holding a rock, these topless females came around, and he dropped the rock right on his foot. He was a real asset.

RV: They definitely didn't go by me. (*everyone laughs*)

KC: Jim Hall hung in there the whole time to, but a whole different kind of thing. Jim is a detail person, but he's such a straight arrow. You don't tell any stories you can't back up. We always told stories we couldn't immediately back up.

WM: Now where did this classic comment come from with this quote of Jim's that "You're trouble is you think the data means something."

JS: It really comes back, as Robin brought up, to the standardized data sheet. It was essential, and yet it was arbitrary, because the systems were different, and some things were reported differently. So the science of streams, and how you understand streams only came through through the data sheet, and our analysis of that, and the cross-comparison. So it short-circuited things. That's what led me to say "If that's all we believe, we're in trouble. Because we're seeing a whole lot more that either we are measuring a different way, or are not measuring, and that's the best we can do for the paper, but I've never been an admirer of straight data that's stands.

WM: That's your genius, too. The ability to look for the big picture, and for the stories. You said it when we started out that the story has always been the foremost part of it, and that's really good. A lot of us took more detailed or...

KC: When we were at the Twin Peaks ranch, when we all got together to work on the same site, the big river site, every night, you and Peterson would have a wrestling match. You'd have a really intense wrestling match, and then you'd both be so tired, you couldn't argue many more.

RV: We also had a number of meetings and trips to decide what major river system we were going to study and look at, and Bob and I went on many of those trips alone because of the cost of bringing everybody east, but we all went to the Alsea. That was in Oregon for the initial study, we went to Arkansas, the Buffalo stream, and the Park Service at that time had acquired major segments on the Buffalo.

WM: Who went on that trip?

RV: Bob and I. We met with the Park Service, and they were very excited at us looking at their system. We also looked at the Pine River in north-central Pennsylvania. A number of streams were considered. But, I guess Wayne would have the best...

JS: But those were your data sites on the shallow part.

WM: Although, that was sort of the tossup initially. We looked at several streams. We looked at the upper Snake and the Henry's Fork. We looked at the Bruneau River. I think it was partly through Jim's suggestion – Jim Brock's suggestion – that I took a look at the Salmon River. The more I looked at it, the better it...

BC: We also made the conscious trade-off on the Salmon that despite the veg-heavy upstream data, we were trading off that whole "river of no return" section that we couldn't get to, I remember we talked a lot about that, plus winter access.

WM: But there were two major tributaries, both pristine, that came in in that no return section. We chose it because we knew we had a system that took us up to at least to eighth order, and dumped into the Colombia, much public land, especially National Forest, and a good part, a core of it in wilderness. It was a real strong driver for it.

RV: In the process of trying to select this list of streams to look at, I asked Luna Leopold what he thought would be good choices of river systems that obviously had low impact, and we wanted one that had lots of USGS flow data and information for our calculations of transport, and Luna suggested the Salmon River, because they I think had five gauging stations, and he was encouraging me very much to look at and select the Salmon, but then interestingly enough, after we selected it, I got a lengthy letter from him saying "Why are you taking the Salmon when that is our watershed?" I had to write back to Luna and tell him "Luna, you suggested I take it." We invited Keith...

VR: Keith Slack? Who is this?

RV: No. The guy from Pinedale...

VR: Lyman? Langmine?

RV: Yeah, Langbein. Exactly. So, I'd been out there at Pine Creek at one of their Pinedale stations. They encouraged us to consider the Salmon, but I didn't have any opportunity to interact, so I dunno why at the last minute, we got this big letter.

WM: At least for that upper part, where they did that tremendous geomorphological work, it was a building block for us. Just a lot of stuff laid in place that we didn't have to go and do.

VR (48:03): Did Leopold have much interaction with the program?

RV: He would come to Stroud lab several times a year – one to teach a course at Penn, which was held at the Stroud lab, and then he came as a part of the academy's review process. Hutchinson, Luna, and...who's the limnologist in those days from Wisconsin?

KC: Hester?

RV: I dunno. Every time Hester went back, his graduate students were doing what we did.

VR: One other person that Wayne and Robin, you've mentioned as well is Herb Ross. When I first heard this, it caught me completely unaware. Wayne's written a little bit about this, but could you comment about Herb Ross?

RV: Well, I first got involved with Herb Ross with his winter stonefly club, which was in the early sixties, I think, with...

VR: Bill Ricker.

RV: Ricker. And I was in Tennessee at that time, and there's a lot of little mountain cold springs that I was interested in looking at other than the Tennessee River system, so I was following the stoneflies there and in Illinois. Then, very luckily, after he moved to Georgia, Ross would come to Philadelphia several times a year because he was on a something review team or something, but he would stay out at the Stroud lab, so I had a really good opportunity to study with him, and I gained a lot of information from Herb – climatic temperature, distribution, stoneflies and caddis flies. I think getting the ideas from Herb on species evolution and stoneflies was really good. We sort of had the same relationship with Luna. I think that opportunities to interact with these people is crucial in developing a bigger picture.

KC: I thought Alabama was brilliant. They said “Herb, just come and stay, and be here.” “What do you want me to do?” “Just be around to interact with people.”

VR: Who said that?

KC: Alabama, when he went. Oh, Georgia. Georgia. “Just be here.” You know, I thought that was brilliant, because that was exactly what you needed from him. He could just interact with people and talk to people, and that was terrific.

RV: And then I would go down to Georgia, and meet with Herb, spin my ideas about how the temperature may affect the distribution of all kinds of stuff. I used him as a sounding board. And he loved bourbon, and that was terrific! (*everyone laughs*)

VR (51:51): Robin, related to the question I asked Wayne. The thermal equilibrium process. That was separate from the RCC. That was something else you were working on at the same time?

KC: Yeah, that’s right.

JS: He had it, but it played. With the mean differences in temperature, they were a lot less in the low order streams, and a lot more in the mid order streams. So I always liked that story, because it did fit the continuum, and that’s where you’d often talk about it.

RV: And American Naturalist accepted that one because we had David. (*everyone laughs*)

KC: That was the key, right there.

WM: It’s a nice trigger. It fits the signals to the organisms very well, and it was one of those things that had a very definite smooth gradient continuum kind of progression over the length of a river system. Basically, I think we pretty much substantiated on dammed rivers and so on. And I never met Herb Ross. My influence was all through his publications. But I’ll tell you, I thought he was absolutely critical. He just helped frame my thinking and bring some of these other ideas into bear, and had a real strong influence in the...

KC: Is this the one where he overlayed each sample – he’s got this paper where...

VR: Yeah. Biomes. Yeah.

KC: That’s a strong paper.

WM: I mean, his focus was a very narrow one. It had a genius in application much beyond what...

KC: Well, the Michigan and Michigan State guys, we all went to the survey to see Ross and Sanderson and all the guys who were there, so he had a big influence on a lot of us early on from his work in the survey. He was paid by the survey to work on leafhoppers, and he was doing the caddis flies in a room in his basement. His passion for caddis flies was independent of what they were paying him to do. He was interested in evolution.

RV: Like George. Mayflies wasn't his favorite, either.

KC: So these guys have to have passion for it.

VR (54:10): Jim or Bert, did you know Herb Ross at all?

BC: No.

JS: I met him once, but most of my influence on caddis was Glen Wiggins, but he wasn't the same league as

BC: Keying out of insects I accomplished or tried up in Canada was using Ross'...

KC: Yeah, everybody used...

WM: All my work on Morgan's Creek was basically using Ross. Ross and Banks, and...

BC: I sat up there in isolation in Northern Saskatchewan, I keyed them up, and I sent them to Glen for verification, and he was quite happy. That old Ross key, that was it.

Group Meeting 3

JS: We were never going to write anything, and then Wayne and others would always ask you things, and it would be very late, and yet the irony on the continuum was when we were really scrambling to get it done at the last minute, I kept the files all organized, and the pieces of the puzzle. But in the nineties, I said, boys, see I have another project, we walked out in the street, and I got run over by someone coming out of a fast food outlet, trying to get on a real fast-moving street, like Cannon or something. And I woke up, in the middle of this four-lane road, and I asked two questions. One was, "Where are my glasses?" I whined about the glasses. And the other one was, "What side of my head got hit?" And the left rear got hit. And what I was asking about was, we had taken these Sherman brain dominant tests to see if the right front was the creative part, and then the right rear was more spiritual. And then this one back here was organization and administration management. And then nothing showed up on that test. So I said, "Great! There was nothing to hurt!" There was nothing there to begin with, and so I felt like I really dodged a bullet, in what could have been a very serious accident.

1:31

KC: But in the end the overdominance of this creative and . . .

JS: So I moved on.

KC: So this follows up with the carrying through from the early

JS: It explains a lot.

VR: So we're talking about communications, and what we want to try to do is get into this question of what made this program—this project—so incredibly successful. Ken is one place. You're in Oregon. Wayne is in Idaho. You're three thousand miles away in Pennsylvania. You're up in "Nuclide" City, and Jim is moving around all over. Robin has said that there was very little money to bring people together for regular planning meetings. How did you guys work on papers—twenty-one to thirty-three papers? How did you keep consistent about what methods you were going to use. How did you logistically handle a project this big, spread out so far without emails, without faxes. What was the key to the success?

2:53

WM: Well, since we keep coming back to this you have to think of other things, but in part, we were somewhat independent. We could handle a lot of this ourselves. I mean, as I said, in our first year, when we were working on individual sites, there was a lot you just had to do. And there wasn't a need for communication at that point. The data gathering point. Other than that you're on task, and that you're doing the same things,

3:15

JS: And you agreed on what you were going to measure and how you were going to measure it.

WM: Yeah. But not a lot of communication. And I'm thinking there were phone calls. Yeah, but I didn't call Ken a lot. Maybe Jim Brock Called somebody at his shop to talk to him. I might have talked to the Stroud area, especially the Peterson lot, or to Bert, since we were doing a lot of things collaboratively, but other than that, initially, there was not a lot. I mean, I had Peterson on the phone a lot, but—

3:43

JS: But that was after we got established. But the lead-up to that beforehand, there was a lot of communication. There were a lot of one-on-one meetings. There were a lot of four or five meetings. And all of those developed a relationship, and I think a trust building that allowed some things to keep going even with huge differences with styles and personalities.

4:11

WM: I'm separating in my mind the communication went on in the preparation of the proposal, and what led up to the pubs and then how those were facilitated. And usually, even now, how pubs are done, there's a leader. Some senior author. And we did some assignments, for example. You're going to take charge of this, and you're going to work with that. And we might have varied from that some, but basically, there was a lead person who maybe prepared the initial manuscript, was on the phone to those two or three people, was using the mail extensively to exchange information. And then you might even hammer out with somebody showing up at your place, like Bob did often, or in a workshop at a meeting, we'd set aside some time at a meeting there. So in the normal mode of transportation, some of those things could be brought together. And then just mailing the manuscript back and forth rather than emailing it. There's no other way.

5:11

RV: I think we had built into the grant one or two meetings at somebody's shop a year. And that was built in. But other than that, it was professional meetings and the telephone. Bob did most of his coordinating of data sets.

5:34

KC: And he's the one we talked to when we wanted to know—

JS: And then he would come around to look through our data sets.

KC: So he probably did more traveling, actually—

JC: In the mid to later stuff.

5:50

WM: And much of what Ken was saying earlier about the email and so on. A good part of that's very divisive. Very time consuming, divisive in the sense of a distraction from your main chores, and from the need of being in the field, gathering the data, and getting into data forms and so on. You can be too efficient, you know. You get eaten up by it. That's number one. And number two, I've been thinking about it a little bit. There's some advantage in having things digested, whether you write them out, or type them out and send them by mail, or talk them over and clarify them in conversation. What you can't do when you blast off an email, and then wish I'd have said that a little differently or more explicitly or whatever.

6:35

BC: I can remember editing manuscripts over the phone. Not by computer tracing and all this stuff, and tracking, but just sitting there, you call Wayne up. Let's set an hour aside. We'd go through the manuscript.

6:53

JS: But I guess it goes back, also in terms of when you read something, you just don't get a feel for it. I don't. And then when you listen you don't. I like to see while I'm hearing, and so the personal, well you could read things, you could see when people were uncomfortable, you could figure a way around it or hit it head-on or whatnot. It was a lot more complete. So to me the gathering is really the most important of how things—

7:25

KC: I don't remember a lot of conference calls. Did we do conference calls?

RV: Yeah, there were a couple of calls.

KC: And when we were proposal writing. I can remember those. But manuscript stuff?

JS: I think we were trying to decide, what data to include at one time. I remember two of them where Peterson had put them together. And we were trying to coordinate what we needed to get in.

RV: I think the most productive times were when we had workshops where we all gathered and divided up assignments, whether it was a main job, or temping, or—

WM: Not only productive, but just intellectually stimulating. I mean, they were real turn-ons.

8:01

VR: Did you involve your students and people that were not directly related in these discussions.

RV: I feel like the Oregon State students were always there.

JS: And you had a lot of the Kellogg folks there, too.

BC: There were a couple of student-related things that I think really stood out. And that was—it didn't involve all groups, but that was the time we went over to Flathead Lake, and there was a bunch of students, and then we went down the Malheur

8:29

WM: And those were not continuum-related at all. Those were very productive, but they certainly weren't part of it, and most of them might have been—

BC: No, but they were to the fact that a lot of people learned what we were doing, and the students got to compare notes from the different places.

KC: It was an OSU-ISU Kind of—

WM: Yeah, I remember that meeting in southern Oregon. But there were some of those. And the other was, I think particularly at ISU, we were stat challenged. You know, we just didn't have a lot of senior personnel. So by the very nature of it, I involved my students very integrally, and Jim Brock In particular played a very significant role in his intellectual contributions, that way.

VR: Do you think a project like the river continuum is good for training students. Is there a tendency to have a student be just a cog in a wheel, that they don't see the whole picture. Was that ever the case, you think, for the students involved?

9:34

RV: I feel the students that were involved, some of them have become pretty prominent in rivers.

WM: I had a couple that I had in research assistantships. They thought it was just a discrete little thing. They put in their time, and they got their assistantship and that was it, and I expected them to live and breathe it and to become involved in it, and I moved away from RA's after that in my program, basically. And those two students didn't really amount to a whole lot in the end. And there were others who just sort of picked it up and lived and breathed it. They may have done it as the job, and their thesis was independent of it. And others used part of the data as their thesis. It was, oh, very very much. But I think it's sort of like NABS in a way. We all involved the students pretty

integrally. It's just that in some shops, there's a more close association with the university operations than others. And Oregon and Michigan and Idaho may be closer and difficult for Bert, and less easily done at the Stroud lab, where they had a whole staff of

10:45

JS: And you had both University of Pennsylvania and University of Delaware.

WM: Right. Plus his crew came out to Idaho. So there were all these young kids with lots of ideas and energy even though they might not have been tied so tight to a university.

RV: I guess Dave and Allen were...came and spent a couple years in Idaho.

VR: This is going to sound so new-age saying this, but I remember as a student, a new researcher, seeing you guys, about how close you are, and how there was this feeling of bonding. I mean, you gave this impression that you were all in this together.

11:37

BC: Complete façade.

VR: Very effective, though.

WM: I don't know that there was any acting involved in it. We respected each other, and each person had a credibility of their own, and they brought something else to the table. So we were sort of dependent on each other. Where one idea might spark and go one way, and someone else could pick it up, or just carry the heavy lifting, and another part—

12:08

RV: I think we were stream ecologists, but we were all our own personalities, and although personalities shift, the common thread is the terrific interest in understanding the ecology of streams, and the interaction of streams with landscapes. So that's really the binding part, and I think that creates the friendships.

WM: Competition occurs when funding is limited. When everybody's out after the same pie. Well in this case, we had the pie. And so we didn't have to fight over that. We didn't have to undercut somebody in the process. We just go on and do the science and be very open and sharing with the—

12:53

KC: I think we believed that we were a team.

JS: Oh, yeah. And we had fun. I mean, we laughed a lot. Sometimes at Bert's expense.

KC: I think the proof of that is that whenever we got attacked from outside, we would circle the wagons immediately. It was pretty clear that there was a mutual kind of thing going on. And at least in my lab, I know that all the students over there whether they were working on the continuum project or not got involved. Because that was the main thread that was going through, and so they were just kind of—Like Milt Ward. He was never really in the continuum, but everything he did every day was somehow related because he got captured into all this stuff. Got dragged out to the field to do things.

13:40

VR: I think one of the things you said too was that all of you were already established in what you were doing. There was no—

JS: I wasn't.

VR: Well, okay then.

WM: I suppose all of us were pretty good, but, uh—

JS: I was riding the shoulders of giants, here.

VR: So four out of five isn't bad.

BC: Watch your generalizations.

VR: There's a lot of allusions to what happened at these gatherings besides writing proposals and writing papers. Are there any anecdotes that you want to leave for history, of things that happened, whether it be Frisbee football, or shooting left-handed baskets, or—

14:37

KC: We were pretty focused on doing some other things. Otherwise we'd go nuts, you know. Like when we were out with Robin, we'd go out to the mushroom farm to see what they really did when you put them in the dark, feed them horse shit, and—

BC: They came back and applied it.

KC: Well, I mean, we did a lot of fun things.

WM: I remember we all discovered the river continuum had been written by hobbits.

KC: No, I—frisbee football was certainly true.

BC: We could spend a whole session on that, really. And we did have a good time. And when we needed to relax we relaxed and we could always come up with High points and low points.

15:22

JS: There was everything, from wrestling matches, ballgames, to a lot of horseshoes at times.

KC: A lot of the stuff when we get together, we'd do an experiment, it'd be like around the clock. Because we would be driven by Brock And so part of the time you're just so tired, you can hardly stand up. You've got that slide of everybody lying around, sort of dead on the couch that's from working on twenty-four hours of...

15:51

JS: George Leinhempher putting up with the camera. And he's the only one standing in the—on the ground with the, uh

KC:... knocked everybody off and killed them . . . And the other guy was George Lincoln He was certainly involved.

RV: There was also a terrific amount of pleasure in actually just doing the work. I think going into the middle fork of the Salmon river. That was just very pleasurable even though it was strenuous and we did a lot of work, spent a lot of time. You know, it was something new for many of us. Yes, I think that the total experience of doing the research just gave you a lot of enthusiasm and pleasure. Because you were always learning new things. I never knew how fast and efficient insects were of climbing over the top of the rock in, water going two meters a second. It was just unreal. And how the bottoms of these crystal-clear streams, you can see so much as opposed to more turbulent eastern streams are. There was just pleasure in doing the work.

17:13

WM: We did some of our first snorkeling as a group on the east fork of the Salmon river. And I remember going up there. And I was sort of standing on the bank where Robin and a couple others were scooting around, and Robin got so excited, he started talking under water. And he came up sputtering, "You know, you should just see all the stuff down here."

17:32

RV: Mid March at the bottom of the Salmon River, not necessarily time to get your jollies, but in any case . . .

WM: We did a couple of field trips while we were working together on the Salmon. I remember one going down to the lower Salmon, down to where the middle fork comes in, and back. And just a lot of sparks going, you know. So we were taking ideas, and they were just trying to classify the stream, how would you classify that, what we would call it, sort of terminology, definitions and so on. It's just a lot of ideas in that short trip of a couple hours. In another case, I remember going up the substream on the east fork, I believe it was. And I really think that Robin and Jim in particular, and probably Ken too, were thinking broader than I was at the time. But they were looking at this beaver influence. Which I really think audibly spun out in what Bob Naiman did and where Bob put in a lot of his work. Those ideas were really from these folks on one of the little field trips. Since we didn't get subsequent funding after that, we never pursued that at all.

18:41

BC: Five years of work, these kinds of things can go on for days, these kinds of little highlights, lowlights. It was full of it, if you'll pardon the expression.

JS: But it did get you in touch with the hydraulics. I mean, I remember when we were trying to decide how to sample with these divers, and we would practice on the McKenzie river, which is a pretty turbulent, fast moving stream. And they were down there, Hawkins and McCullough And this guy was kind of fishing the bank, looking at them, and wanted to know what we were doing. And we were just kind of trying it out. And he says, "You guys are nuts." And he says, "You wouldn't catch me dead in there. And we starting talking to him and ask him what he did, and he rode whales down a thousand feet. And he was thinking that there was no way he'd get in a turbulent river with a lung and face mask, and all that on. And so it kind of gave a different perspective. But the perspective to me with you looking at these fast-moving bugs all kind of magnified was going in and going into a pool in the lower Salmon and looking up, and there are standing waves above you, and you're just in still water. And so I never really realized, I mean you'd read about it, but you'd never really felt it or experienced it, and it was just sensational.

20:10

WM: Yeah, that really—what you observed and others report on gave us some insights in trying to interpret the eighth-order data, in terms of what we were seeing and the whole overriding dynamic of the transport, as opposed to the non-transport parts of the system.

20:30

KC: And that was deep water, too.

JS: And you'd follow the sponges from green to white.

KC: You know we had a bottom-sampler. The thing with the glove box with a big pump in there, and so.

JS: Now who was that—who was that Bob Gayle, or uh . . .

WM: That's who invented it.

JS: Yeah, and we went up. I think, was it you and I went up and looked at it the first time?

KC: Yeah, was it? Okay.

JS: We bought one of those to try out, and it never really panned out as well as we wanted, but—

KC: The guzzler was the thing I remembered. It was the main piece of equipment. We had a giant pump that you pumped a little water up. We could pump rocks that big through it. Put the tube down and pump the damn sand out into fifty-nine cents. And that was—

21:33

JS: But I think that also at that time, because we were trying to figure out how to do things, there were tremendous advances in technology. And I look at the chambers as one that just—really, nobody was really doing much. They were dinking around with that. But the collective of Bottt and Brock and Gregory. And using stuff that McIntire had done and modified there. I mean, it moved at a whole long way fast to get at a specific question. And so . . .

22:07

RV: Yeah, real method development.

WM: So at a much simpler level, the considerations about the transport and the BOM and the fractionation of it, and I think that some of that came from your earlier work on the detritus break-down some, but still, nobody had really attempted to characterize classes of the FBOM and CBOM and the BOMs

KC: Yeah, that's right.

22:40

BC: Which we actually carried on through our next proposal with the C-14 stuff. We'd predicted this from the continuum. Didn't quite hold. We had to figure out how to go on to the next step and see how far the stuff really went. It spun off that next proposal.

22:56

KC: So with Stroud and the Kellog we were running these thousands of these Gilson respirometers. I had a full-time person, and all she did was run those. You know, there were two machines she'd set up every morning, and get in two runs, and oil the things overnight, and do it again and—

RV: Maybe all that wasn't so necessary.

KC: Yeah, exactly. You know, we did a lot of that to try to figure out—

RV: Energy budgets.

KC: Exactly.

23:23

WM: One place where we would have gone or didn't go that I wonder about is the whole DOC aspect. And it may be that we were just limited by the technology of the time. I think that Rick Larson Had some really exciting things with the disappearance of the really non-recalcitrant materials early-on, how quickly they went, and how far the recalcitrant materials went. So we weren't really able to look aspeciate, look at that in a lot of detail without some really elaborate chemical—

23:56

KC: Was Cliff Dahm Involved at all in the—

JS: Cliff was an evident peripheral.

KC: But he certainly got some ideas. I mean, his whole thesis was on—

JS: Oh yeah. His thesis looked at DOC and some diurnal seasonal patterns in the flowstream but also from the Bonneville Dam to the mouth of the Columbia. So he was looking at DOC at the bottom end. I mean, he had Portland in there too, but essentially he was taking an oceanographer's view.

KC: But the lab experiments he ran, some of those came right out of continuum ideas. Where you get this initial physical take-up that takes place in minutes, and then you have this biological take-up that takes twenty-four or forty-eight or more hours.

24:45

JS: And then we did a bunch of stuff that wasn't straight off the continuum, but it really followed up, looking at beaver ponds, and steppes below landslides, and you get this anoxic and you get this release of nitrate phosphorus. And there're hot spots where there'll be a lot of action, lot of nutrient turnover, a lot of algae.

25:09

WM: And a lot of this was set in the framework of the four back a rig, the interplay in the Salmon, that could be a fairly significant thing, and we just sort of weren't even aware of it. And have this sort of move back on it.

25:27

KC: Well there was Lienkempper, and whoever worked with him that mapped the wood. And all the sites, right. So we had this idea that that was certainly a—and if you look at the inter-biome comparison, they've all got the maps with the wood debris. So the idea that wood debris was something to compare. And Lienkamper was the one that made all the pickup stick drawings.

25:51

VR: How about your studies on the Wilamette and things. Was that influenced by the continuum, on the wood removal, and the documenting—

JS: Well, that got into just history, and part of that—You know there's a lot of things that come full circle. And some of it came out of you couldn't miss the large wood in these coniferous streams. I mean, you'd be traipsing over six-foot diameter fir or cedar on your way to the site. And we didn't pick up on it early on, but we were on to it by the time the continuum came on. And then that nudge we got was from Liken's group, cause they were doing it. And then Bilby at the time came out with his thesis, looking at the role of wood in retaining materials in the Hubbard Brook. And so the thing was—this was just a west-coast phenomenon, these big coniferous forests. And I said, like hell. I mean, just theoretically, any time you have a forest influencing, something, they've got to fall in. So then I started looking through old records of the corps of engineers snag records. And I started working back from large streams to small streams. And then you got into splash drives. And you could find all these little—it didn't matter whether you were on the lower Colorado, on the Kissimee, I mean you had us do some stuff later on. But the story was all the same in terms of edge structural habitats, side channels, and things. And so we felt it was just a story that was the same. Europe lost it a couple centuries before we did. And we still had parts of it there in large rivers, but by and large, everyone else was missing that. And then that started to a whole different thing with microhabitats, and we moved it to salmon, and salmon habitats. But the historical stuff certainly was looking at that. And that was going back to Charles Lyle in the 1860's, and Marsh, and they'd talk about these big rafts of wood that come out of the great rivers of the world. And so since you were stringing it all back to the little streams and . . .

28:10

KC: Well it became clear that most of those streams had been snagged. I mean, they consciously for a century removed wood from streams. And the reason the west was different was they didn't get around to all of it yet. But they were working on it.

JS: Oh yeah, right. And in fact the stream, at the time, ninety percent of the money for stream improvement, well it was a channelization story in the south and in the Midwest and the east. It was a fish habitat. And from the early thirties, everyone felt that these big debris jams were an impediment for fish passage, and if you only got rid of them, the legend went in the 1890's and the fish wardens, that the fish would have more energy to get up and go back, and they wouldn't die. And it was just a thing. But the knot then came on fish passage or partial. So anyone that looked at that, it was a negative. And ninety percent of the stream improvement money was to blow up beaver dams and to pull wood out of streams. And so that was an impetus to look at it again too.

29:27

KC: I remember we went up a stream without Robin and one of those, and you pointed out that well you know, this is good. This is arguing for a long long time that all these streams were degraded forever. And now lot of this agin lines being converted to other things, and streams just turning graded. And some of these old trees are coming back up. They've been buried down in the sediment forever. And like wormy chestnut. Worries about the millions of Square foot is coming back, getting exposed again. And so we started looking at Michigan for exactly that same thing, particularly since the Chestnut blight went around it. And white pine was just gone, but there's a lot of streams where there's actually White Pine that's down in there. That's down, once these streams start to degrade after this long period of sediment in, only sediment in for all kinds of reason.

30:21

RV: Yeah, a lot of trees are appearing again, White Cedar's another one that's reappearing.

KC: So I thought that was a really neat, interesting story.

RV: I think some of the interest in looking at this large wood that we could easily see, especially in the Andrews. How they were structuring the drop of the stream with their steps. And retaining again. And we were also aware of Leopold's work on the uniformity of energy dissipation in the channel. Streams don't accelerate if they go down stream appreciably, but there is this trade-off in energy dissipation. Energy dissipation is very important in streams to prevent erosion. And stepping streams down progressively is an important way of dissipating energy. Pull sequences in honor, function, channel, Lisbon, design and flow. And I think looking at how this energy that's stepped down is really important, and trees have always been a part of balancing the landscape to floodplains and slopes. And I think this evolved out of our studies. You get the linkage of streams in the landscape and the role of vegetation, and the controls, you know. It goes with a very important contribution of this project to the management of forests, especially in the pacific northwest. It was a Stiltzer-stienburg

32:20

WM: Yeah, you know it was a total paradigm shift, and you know from one of removing the wood as you start outsidings to one of protecting and encouraging.

JS: But it flipped around also to a recognition, where before we always talked about fish-bearing streams, as if they were separate, and now you had had fish where you didn't, as opposed to a network that was fed by a real intimate connection with the surrounding forest. Then that moved on up into northwest forest plan, where the acknowledgement of intermittent streams in feeding this, both for habitat, in the form of wood that would come in, but also in the form of fine particulates, or other partially-digested materials, or just refuges during high-flow of going in in those interstitialaries was huge in city-side repairing buffers.

VR: Well, I want to make sure we get back to this tomorrow, cause this is really an important point.

Group Meeting 4

VR: Talk a little about the research that was done. Yesterday you mentioned that the first grant largely concentrated on you doing your individual study sites using the same methods, and the second grant was the large study that was done on the Salmon river. Could you tell us a little about the research component of this and the number of papers that came out of this indicates that this was really a very strong part of the RCC. It wasn't just theory. It was actual hypothesis testing. So maybe we'll talk about the individual sites, and then concentrate more on the big Salmon river study. Anybody like to open up?

0:43

RV: Well on the White Clay Creek we selected three stations. A first order, second order, and third order site in a woodland setting, a small eight hundred acre...hectare site, and then to get larger streams, we had to move over to Doe Run, which is a large holding on the King Ranch in the Brandywine drainage system. Unfortunately the White Clay as it went downstream received too many impacts to be a natural system. So that was one variation in the system. It wasn't the same river system. But it seemed to work very well with an adjacent watershed. And I think the parameters that we measured during the primary production that Tom Bott Was instrumental in designing and working on that part of the project. Benthic activity, benthic organisms and the detrital component inputs and standing stock and transport. I think it was the standard methods that we all used across the biome, and at times we went to each other's site to see what was—how they were performing and what their sites looked like. So we had interactions during the course of the study. We all visited everyone's sites.

2:23

VR: How many years did this first phase go on for?

WM: Well there was the two year award . . . so two years and a renewal. So four years total.

VR: The renewal being the Salmon River?

WM: Yeah, yeah. Well and cleanup on some of the other sites, I think. And perhaps a lot of data processing and manuscript preparation in that period.

2:56

VR: Wayne, could you talk about the sites that you worked on?

WM: Yeah, our sites were on the Salmon river, although we spent some time just looking at several locations in Idaho, in Southern Idaho in particular that were accessible from Pocatello at Idaho State University. And we initially chose the Salmon for a couple of

reasons. One, because of the continuity in the system, it did go all the way from a first to an eighth-order system, and we thought that was quite attractive. And secondly, there was minimal human impact. There's a substantial Federal holdings, forest service, wilderness area, and other land, and relatively little development. There were a couple of towns, mostly small towns, Salmon Idaho being the largest, and that was way down in the lower part of the system. By the time, it had already attained an eighth order in size, or seventh order at least. So after that, our decision was to work with the upper regions so we would match pretty much what was going on at the other locations in terms of size. But we also tried to sort of push it a bit downstream to get it to the largest system that we could handle, and I think that was maybe comparable to the McKenzie river site in terms of its current velocities and depths and widths and things of that sort. And then other than that there was some more development of methodology that was specific to our sites, and one of our folks, Jim Brock, took the lead in devising some really innovative transport sampling techniques, and I think that those ultimately became sort of commonly used or at least it was a number of really innovative ideas that went into his approach, and were useful when we upscaled to a larger system as well. Other than that, I think we handled most of ours, because we did not have the depth in terms of other people. We did most of our actual sampling and development in this phase with graduate students. I had two supported directly as RA's, and a couple of others that were partially supported. And in particular, I think that as we mentioned earlier, Jim Brock's name keeps coming up, and he was a master student at the time, but well ahead of his time in terms of his abilities, and he made a tremendous contribution to the program.

5:36

VR: When you had these innovations in the methodology, how did you pass them on to the other group, or was it really intended to wait till the Salmon?

WM: No, not at all. I think there was a lot of phone things. Jim was a very outgoing sort of person, so a lot of this was passed on, probably by phone. But also, then, the visits, we would showcase those sorts of things. And I remember on several of these meetings where we would meet on a river bank, basically do a show and tell, as part of the operation. And in some cases test out the methods there, other cases set up a workshop that went after those in some detail.

6:17

BC: He was actually building the chambers.

KC: Right, yeah, that's right. He built them all.

JS: From those workshops you'd start an idea, and the methodology development was an intense interaction, and there was a normal amount of arguing, so there're a lot of parallel testing. We'd check it, and see what it was, and work it. And that was some of the most intense of discussions on how we're going to get an accurate measure, and what it'd take

to get an accurate measure of say primary production or respiration or a particulate organic matter.

BC: The detritus workshop, even though we finally settled out on a particular apparatus or device, it was more on the sizes, and how you got the right amount of water through it and got the sizes you wanted.

WM: Yeah, in fact up until that time, there was no routine wide-scale partitioning of particulate organic matter? This was really the first major effort. And it really has set the stage in what people have done. There might have been some refinements along the way, but for the most part, those size categories have held up and found to be functionally important and accurate.

7:44

KC: Those chambers made it out of the continuum project even then. Dick Marzalo's students on the Konza Prairie built chambers, with Jim's help built those, and those chambers were running in streams on the Konza about the same time we were doing the Salmon. So this stuff, Cathy Tate and a bunch of people were actually using the same design, and those papers are all over the place now. And Jim's continued to—I mean, when he came to Florida, they were completely new designed, remotely operated valves, and all kinds of stuff. So he just never stops.

8:22

RV: Tom Bott's Chambers have been used this past summer in the Catskills and New York State by Tom and it's true. Measuring . . . This is a technology that developed, and has really continued.

8:42

VR: One of the things, before we go on, what were the years of these grants. Do you remember like the first two-year grant?

BC: We got notification that we got the grant in seventy-five.

JS: In the fall or something like that

BC: Just a second, let me get the . . .

KC: My sabbatical was seventy-six?

BC: July of seventy-five. We were meeting in Pocatello and had the grant in hand. So that was when we really had to . . .

KC: Do you remember what the amount was?

BC: No I don't. We could dig that up, but . . .

KC: It was big, but it wasn't huge. I mean, was it a million?

JS: No, no. I think it was a few hundred thousand, spread over course of . . .

9:41

VR: And then there was a Rockefeller grant that brought all of you together initially, correct?

RV: Only to provide funding for travel for thirty to forty people.

VR: And what year was that, Robin? Do you remember?

RV: I think it was fifty two or—no. Seventy-three or seventy-four. That was initially outside of the—

VR: That was a very important meeting that got . . . And then the second grant would have run from . . .

JS: Seventy . . . seven to eighty or something like that.

BC: Yeah, cause it was three for five years. The total is five. Two and three.

10:25

WM: And remember that there was a smaller grant ahead of that that provided for the working and the interaction. That was NSF. So we had sort of three NSF's. A small one that we don't really count, in preparation, and then the two major ones that were the river continuum project.

VR: And the second grant? That was the one that was about a million?

WM: It was just over, I think. As I recall it, the first one was around eight hundred thousand. The second was like one point two.

BC: I always thought we totaled pretty close to three million for the five years. That's the figure that always came to my mind, but maybe I'm wrong. The total for the whole RCC for five years was just short of three million.

11:16

RV: I think that what's always interesting, however the amount the money was, there was never a squabble over the partitioning of that money, and who needed people for

different projects. That flowed very smoothly. Everybody knew how much work it was going to be. And I think a lot of work was actually subsidized through other projects and other monies. And perhaps the major shortcoming in my mind on the funding part was that the project would have been much stronger if we had sampled more frequently during the year. This was sparse. And Wayne's crew had some very rigorous areas to get into at really difficult times of the year.

BC: I was going to say, frequency of sampling was a lot different in your shop then up there in the mountains cause it was—

RV: The logistics of Wayne's effort, and actually the Andrews didn't have an easy time.

12:28

VR: So let's hit some of the other sites. Jim, you want to mention the Andrews?

JS: Well the Andrews, we anchored that in the Andrews Experimental Forest. And again, our looking at choices, it was easy to find the first five orders, and then we chose the McKenzie which hydrologically is very different, but it's Lookout Creek Flows into Blue River flows into the McKenzie river. And so the McKenzie was our big site. And we had no clue how we were going to work that. Cause it's just a big honking river. But the others could play off of work that Fred Swanson, Frank Triska, Stan Gregory were working on. So I second what Robin was saying. We had a lot of subsidies. A lot of our technology, innovation, and the work, both on detrital respiration and that came from people. We weren't funded at all by the continuum. Stan Gregory And Frank Triska, we might have paid travel for them. But their contribution was over and above. And I don't know if we paid Bott a whole lot.

13:35

RV: Right, he was totally subsidized.

JS: So we had this network. I think everybody's group had folks that we drew in just for the excitement of not only the methods but trying to get that accomplished. So we chose to go with a post-doc , because the main crew at the Andrews were pretty tied up doing things for other NSF grants, and the IBP programs were kind of winding down, and so we got a post-doc lead and a new crew of technicians to do that. Some of them have gone on, done some pretty interesting things. Naiman was the post-doc lead Bob Naiman And then, Chuck Hawkins, Dale McCullough, Karen Lukessa we hired as technicians. And then we drew on people like George Lienkemper who worked with Fred Swanson And I think if we had something that we pioneered and did cause it was an extension of what we were doing in the Andrews, was measuring big wood, mapping it, and trying to look at how that might shape habitat and some of the features of the streams, that went off to the other sites. I know George was on the main Salmon as well as at the . . . But our approach was not unlike the rest of them, only we chose a post-doc lead

15:12

VR: Are there are any other sites that—

KC: We did the Kalamazoo all in the same basin. A first order and a third order site which is in the Kellogg forest, which is a part of Michigan State's holdings. So we'd been working there already. So that was one of our Fizz. So we did one, three, and five. The fifth order site was on the Kalamazoo above where the dams are around Battle Creek which is the Kellogg Company area. And what has Robin said, the subsidy, and Jim as well that all of us, everybody in my lab was involved in the continuum project. Including guys who were working on salary, something about a curiously strong mint... So everybody understood that when we were doing continuum work, everybody did it. And so the techs for a lot of my people were in zooplankton work at the time for NSF. And so it was just understood. And there was no complaint, because when they needed something, everybody—I mean that's just kind of the way it worked. And Roger Ovink was our guy that was doing the primary production in the continuum part, but Donna King was doing a thesis using her chamber for primary production, and she was looking at sites, little windshield-washer motor driven off a battery that was stirring the chambers. And so everybody had kind of their own thing, but when it coalesced into these beautiful chambers that Jim was working on, everybody kind of got on board with that. So it was definitely a subsidized team effort. It was just understood that this was the most important thing going on and everybody had to take part. I think that our contribution was because Mike Klug was there. I mean, he was really interested in the microbiological aspects, and I think he was the one that said there's something different microbially that happens at a particle-size of about a millimeter. When you get down to a millimeter, these fungal mats cannot maintain themselves on particles like that. And so the hyphomycete fungi were really in coarser stuff and then it became a bacterial show. And so the biochemical tracers they used showed that. But then he got us involved in using Gilsonn respirometry To track the respiration of different particle sizes. To see whether there was some signature that—well the sizes we decided on which I think were ecologically based and sound, was did the microbial stuff track that And it did. And we did a lot of experiments with . . . His technique was to kill the stuff with ethylene oxide And then see what happened without the microbes. And that was all kind of lab-based. And so Klug put a lot of time into continuum ideas, and he was totally supportive of the things. So I think without all this peripheral and subsidized, we never would have pulled this off. And Robin's exactly right. We needed more sites and more times, you know. So we can be proud of something. We did a heck of a lot with a kind of constricted resources.

18:53

WM: One place where we did not go, which I think I've always think that we've covered inadequately, was in the benthic production site. I mean the real further tests of the river continuum was to know what the invertebrates were doing. And we had to back off of that. We did not have the manpower, the people to work on it, we did not have the

funding. So it was both a conscious decision and a group decision to say we were gonna leave that untouched.

19:21

RV: Yeah, all the money could have gone into that one area to do an adequate job.

VR: Was there research being done at other labs that you were drawing into? Like for one thing Stu Fisher by then, was he in Arizona?

JS: Yeah, but he was en route. I mean, he was just leaving Amherst and setting up shop in Arizona State. But I think aside from—I mean we used a lot of Fisher's stuff at the Andrews. In fact, that's what when Frank Triska and the rest of us published that nitrogen budget. I was really trying to refine to our system and do just a budget. And we were really playing off Bear Brook. And we were obvious, and Stuart knew it. I mean, he helped us and whatnot. But when we got to the continuum, I quite frankly didn't see there was that much coming in from the outside. Most of it was generated inside. And that would people from the Konza and many of those who'd started or been associated with one of the other labs, and then they would take off on a different tack.

20:40

VR: You see, you're generating new research, rather than . . .

WM: And again, we were children of the IBP, so this whole energy budget focus and nutrient cycling focus was really that was an IBP high point, I think. But also, if you're studying eco-systems, that's where you go. I mean, those are ecosystem level parameters. And that's where you start to focus. So the rest of it was the embellishments, the petitioning, the whole new directions that were taken, I think came from within the group. And what was exciting about it, I mean, that's where the sparks were. And it was, I mean the IBP on energy budgets was really a place in space and through time that, I mean you had a weird pullage or something. And certainly now you were doing something on a longitudinal gradient. And we didn't have a clue as to how to adequately do it. We knew we had to arbitrarily choose a spot to do it, and we knew that that wasn't quite what our vision was, and this is what got into our—there was this tug-of-war between the necessity and arbitrariness of coming up with the same measurements, and also of picking a site. And knowing that there's a lot of variability from first order to fifth order, and where do you choose that site, and how do you integrate things along there. So that that stimulated a lot of the excitement, I thought.

22:11

VR: Bert, you want to add anything?

BC: I was going to say, it's almost like arguing against what we were trying to prove. By picking arbitrary sites to prove a downstream thing.

VR: Let's talk a little about this second grant, the work on the Salmon. Where you brought all your developments from the first studies into this one river system. Robin mentioned a little about why the Salmon was picked. Of course, Wayne, you had been working on it. Could you talk a little about that project?

22:39

WM: Yeah, I'll start maybe. But one of the things, the overall driver is, we're trying to get into the bigger systems. So we were trying to extend beyond where our individual locations had gone. And we felt the only way we could handle that operationally, logistically, was to marshal all our forces on that particular system, whatever it might be. And because of some of the things that were mentioned by others, where they ran into system changes, or in another watershed, or pollution all down, I think the Salmon came out clearly as the favorite place because of the lack of those other confounding issues. But that gave us a big system and one that, as has been mentioned earlier, is difficult to access, sort of our lowest site, our eighth-order site, down near Riggins, was near the other end of the state. We had to go all the way to Boise and up north just to get to that site, so it was a day's drive basically to get to the site to do it, and so we took it sequentially down the system rather than doing things simultaneously at all the sites, especially in that lower one, because of that logistical problem.

23:54

VR: Were you worried about seasonal differences?

WM: Well we had always been worried about seasonal differences, and we tried to do the four seasons, but when you get into that lower section and the logistics and so on, and then bringing people from across the country, where the, you know, land-based travel. Robin came all the way with trailers and sort of a wagon train of folks to get across there.

RV: Thirty-five people. Out there in March. Late March.

WM: So we looked at three seasons, basically, Early as we could do it, the summer, and then a fall if we could swing it.

24:28

VR: So you brought people in for weeks at a time each season.

JS: It was a three week pulse, essentially.

VR: Yeah, three times a year.

RV: Built a lab there and distributed the work station

KC: Remember Big Red that did it up at Oregon State? Awful pickup truck with a huge trailer on it—

JS: He bought a pickup truck to—

KC: Make fun of—and that truck ended up—the title ended up in my name somehow. I was the owner of that damn truck. First the damn truck, and then there's insurance, and oh God.

JS: It had a camper unit on it.

KC: Yeah, and owning it really—The camper was great. It was battery-powered, and all the chemistry inside and—

25:10

BC: Can I stop just a second? I think there's one significant event that we ought to talk a little bit about, and that was the Middle-Fork float trip. And that sort of came in before we started the big push downstream, wasn't it? It was in seventy-eight, and

WM: My remembrance of that is that we were doing the Salmon, the main Salmon, and we did the Middle Fork, as an aside kind of to explore some other questions that we had.

BC: But whatever it is, I don't want to miss that. I thought that was pretty important.

JS: Because I left the continuum—

WM: Well, can we come back? I think we ought to stay focused on the purpose.

JS: The main Salmon is really the second part of the River Continuum. The Middle Fork is a sideshow.

WM: So a couple of things that we need to mention here that are important as well, that this is when Bob Peterson Came out. So he'd been the coordinator and based at Stroud And he came out to Idaho and set up shop there and basically did a lot of the legwork to see what the sites were, we went out together and kind of selected the final locations. Chose a central location within the Salmon River Basin that we could rent housing and food producing facilities and a lab, and to get that set up, and then some of that was transmitted back, like George Spengler I know it came out with all of the benches and everything pre-fabbed and the hood pre-fabbed, and they just assembled it on site. So it was a major logistic kind of operation, in which Bob was playing an extremely important role in doing the coordination here. Also there was this whole deal with the land holders and owners and people like the forest service, and we had to coordinate with fish and game people so that they understood where we were going to be, and what was going on, and what the commotion on the river was, and they were clear on it. And that took a little

bit of public relations and outreach in terms of just—We made a brochure. I think it was floating around yesterday, that was to inform people what was going on.

27:22

RV: I think there was a newspaper article. Prepared the way and again, that was really—

KC: One logistic thing that shouldn't be glossed over was that when you're talking about the numbers of samples and things, the labeling issue. So that's the only thing that Spengler really—I mean he assembled a lab, but he also brought the labeling issue. And that became a real issue. Getting all these samples labeled, I mean that seems trivial, but by god, that's not trivial. I mean, just keeping track of—I remember he had these thousands of bottles.

27:59

JS: These report barcodes

KC: Barcodes sure would have helped.

RV: And we had another challenge to keep all these generators going twenty-four hours a day to run that PR chambers in the cold weather, or any time. And we fortunately had a fellow called Bill Shaw who was just one of these guys that could make any motor run and any electricity run through any wet circuit. To keep all these flow chambers going, you really had to have a mechanic somebody in charge of every detail.

BC: I've had more than one person ask me when I show a slide with all the chambers out in the Salmon and all these wires is, "How many people did you electrocute?"

28:47

WM: Why don't you tell them?

RV: Right, and somebody had to design a trailer to dry the wetsuits so they could be put back out without somebody being cold.

WM: Yeah, so a lot of the lower section or—maybe all of it was done, the invertebrate sample, was done with divers. And even the placement of the chambers. So we had a whole team that had been trained and had practiced in various such situations so that we felt comfortable with operating in that kind of—

29:21

KC: Talk about Scott, too.

BC: Yeah, Al Scott we brought. He was a trained diver in my lab up in Washington. And even though he wasn't associated with the continuum or anything, but we got him on to this diving, his expertise in diving.

RV: Oh, it saved people's lives to have him there, for sure.

BC: Yeah, he was real key to just integrating the whole diving program.

29:43

VR: Did you have any serious accidents during any of this?

JS: None that we'll talk about. No, we almost lost Bernie.

KC: Ruptured eardrum.

RV: Right. I went home for a fifth lumbar operation.

JS: Well see I remember one of those, on the lower river you got used to—and the diving was tricky if they were trying to hold on in a really fast current, and I know that we had one person had to just drop out his weight belt, and got tumbled over and bruised up a bit. But for the most part we didn't. But you'd start to be lulled into thinking you knew something about the river. And you'd look at a pool, and the current would be going down. And you knew if you got out about fifty feet, got down, the current'd be ripping, and you'd be in dead water. And so occasionally, you wouldn't start at the head of a pool or a run. You'd start in the middle or the lower third. Big mistake. And you'd get out and sometimes the run never got deep. You went through a whole air tank and panicked trying to get across the river before you hit the rapids below. And there were a lot of things that was trial and error learning.

31:09

WM: Ominously, one of our sites was called Dead Man's Hole. And it was maybe the first one down that we started working with so it was shaky on occasion just for that.

VR: Did the research go as you predicted? Were there things that just constantly came up that you had to do modifications?

BC: Oh, you don't do anything at these sites without running into little things.

31:34

JS: I think the biggest thing that we were . . . We knew there'd be an issue, but it became a big issue, was just people. People issues. And it's integrating four, five crews who had been working very intensely with each other, and had worked out a rhythm, and how to accommodate people's strengths and weaknesses, and then suddenly you're jammed for a

very intense thing where you're going to run sleepless for three weeks, and trying to get that going. I think that was a huge issue. And certainly in our crew, we basically left our lead person back home to write papers on our site, because it was not going to work to have that individual interacting and trying to constructively make the bigger picture go. More on the PI's. There was some flare-ups and so on which we got by, but still that's a real triumph.

BC: I still have one slide of all five PI's working at the same table picking bugs, which is a rare slide. But it proved that we could do it.

32:47

JS: No, it was the people issues, and how we tried to accommodate that.

BC: An example of that, we had a little spat between the divers and the bug pickers. The divers go do their thing. The bug pickers then have to work into the night. And they got a little unhappy to see the divers out there tossing horse shoes. But these things worked out.

33:07

VR: Did the results live up to your expectations?

WM: Yeah, I guess, with the missing macroinvertebrates stuff, it just leaves a big gap in terms of interpretation. I thought, although it's long in coming, that our eighth order paper was a very good, complete analysis of what we set out to do. There's obviously lots of things we could do differently. A lot of understanding that's going on now that we could have applied if we'd had that background, but given that this was a pioneering effort, I think it worked out pretty well. We didn't talk a little earlier . . . For me, the sum of it turned out to be more complex geomorphologically than what I had originally anticipated. So we sort of did not go through the kind of classic stream changes, in terms of canyon, meandering braiding kinds of sections. And below Salmon, we get back into a canyon constrained section for a very long stretch. So just dealing with that conceptually and trying to sort of handle that in terms of what we originally set out to test. That added some challenges to it.

BC: I think the very fact that we're sitting down and talking like this indicates what it did. We hadn't have been pretty successful in putting this paradigm out, we wouldn't be sitting here recording it.

34:34

WM: Well, yeah, and not to sagehook, I mean, what I stated earlier. There are a lot of critics out there that can take shots, and I think some of those are well-deserved. There are gaps. It's not a fully documented test. And yet it may be the best shot that's ever

able to be marshaled, given the funding environment, and the changes in direction that some of the research is taking now.

BC: Likewise I'm sure there'll be people in the future that watch this tape and say, "Why didn't Resh ask this question?"

35:08

JS: I mean you'd think after twenty-five years, the field should be staffed with smarter people than we are, and that we'd learn something in twenty-five years, and yeah, they'd make huge changes or calibrate it to these different parts of reaches of stream or said something. I mean, if we haven't, then man, they're not doing their job.

35:35

WM: Well and you know, you'd like to think that budgets would accommodate things. Just take an ideas that you're going to do research very well. Multiply it by four, because that's the number of sites we were handling in a given year, and then multiply that by three to four, because that's the number of sites we were doing across the nation. You've suddenly have magnified the budget so that it's pushing the limits. Now if you want to add, which I think we would have done if we could have done it, instead of looking at transects, we would've looked at up-down sections. And we would double your budget right there. And then if you add the hyporeos into that, which we did not appreciate at the time, but which we now do, you'd add another component that had to be covered. And suddenly you're really pushing the limits of physical and financial ability to do some things.

36:18

KC: And then if you want to deal with floodplain issues, you have to have a river with a floodplain. So the Wilamette, or something, would have been—and of course it got isolated from its floodplain a hundred years ago, and where're you going to find the site to do this stuff?

36:38

VR: Well I guess the next question that I would think of is, once this grant was over, did the result influence other research that each of you did. In other words, did you stay on questions of the continuum that had come up say in the Salmon river study? Did any of your research go directly in that?

BC: Yeah, I think one of the main spin-offs, at least for me and for Wayne was the one problem and question that we generated with all this other work, was downstream transport defines some things. So after the continuum was over, Wayne and I pursued this with the Carbon-14 tracer stuff.

37:15

WM: Which was also an NSF funded grant.

BC: Yeah, another NSF.

WM: And the real key issue here is that if there's a continuum or progression, then the particle that starts in the headwater has to make it all the way through the system. And there was no way of verifying that, and the alternative would be that it's totally metabolized within a very short reach of stream, especially if you have a high retention. And what we're showing is kilometer levels of movement in a day's time. And easily projected, that could clear the system in the course of a year or less. So it's crucial to validating some aspects of the continuum that we couldn't pursue.

BC: And just as an example of how little we knew about this, when we were writing the grant, and trying to make up our own mind what length to look at this, we called a whole bunch of guys and say, "How far do you think a particle under these conditions would go" And the guesses were everything from three meters to three kilometers. That was everybody's impression.

38:17

KC: Then there was the coarse part of that, where we, based on some things that Robin had said early on, we now as a standard thing, use ginkgo leaves to track litter retention. And we do it in our classes all the time. And that resolves at hundred-meter reaches. You know, leaves, they don't go kilometers. They go meters. So there's a thing that happens between coarse stuff and fine stuff. So that's just kind of for us become a standard thing to calibrate different reaches for their retentions for the litter part. And ginkgo is great. They all come down at once, they're all yellow, and nothing eats them for a month. And they stay yellow in the water for months, so wonderful tracers.

39:01

WM: I mentioned earlier that particle size differentiation has just pretty well stuck as a standard into the dynamics now, and then although it started, I think with Ken's shop, it was integrated into the river continuum, and is really now very common that the litter decomposition efforts on putting packs of leaves out, looking at the rates of decay, and using that to sort of document microbial activity. And then Jim had mentioned earlier, the wood. That's been a big issue. That was a whole paradigm change, as we talked about yesterday, and just the very fact that people are documenting, keeping track of that, and considering that in terms of retention, which in the meantime, the whole spiraling concept evolved. We were able to apply that and test that with our data in the eighth order, cause we were set up to do that, and what's absolutely remarkable is that those predictions made with a very crude model at that time, now are being validated by the latest publication that Denis Newbold is senior author on with the elegant models that we've developed in relation to the FPOM transport.

40:15

VR: The spiraling idea came about independently, though, didn't it?

JS: It came really out of Walker Branch at Oakridge

WM: Independent, but we felt that it rode on the ground cut by the river continuum. I mean, spiraling would not have even been germane if there hadn't been the idea of continuity. If we had been thinking conceptually in terms of segments or zones in the old European fashion, there would have been no spiraling concept.

40:48

JS: And then it also stemmed, again, out of the IBP, where they were into nutrient cycling, and how do you translate that into streams, which don't at a point cycle.

Group Meeting 5

VR: Ken, tell us a little about some of the spin-offs that you had from the river continuum and that affected your research for the future.

KC: Well I think one was we started trading leaf packs around the world. And that's how I got to know Ian Campbell. He sent us eucalypts and we sent him roadies, and they're both "shoe leather." And so we started looking at the temperature directed rates of disappearance. And it turned out that his bugs and my critters recognized rhodies just as they would eucalypts, and our bunch recognized eucalypts just as they would have roadies. And they all went at the same rate. Temperature corrected rate. So then we traded some packwork with people in Italy. That's when one of Bonnomi's students came over and worked with us in Maryland at that time. And then there's a lot of leaf trading going on now. Leaf pack trading.

42:23

VR: Yes, there's a big group in Plon that are running it.

KC: Yeah, and then one of the other spin-offs was one of my graduate students that did a PhD with me, had done a masters with Jim Barnes at Brigham Young. And so that got Barnes involved in the—in fact, Jim Barnes came to Kellogg and started to do leaf-pack stuff there in lakes. So there was a lot of sort of leaf stuff. And then I think the main spin-off for us was we mounted a big experiment to try to get at the issue of what the best analog was for leaf decomp. And so we had a tributary where there were, I think two Basswood trees in the whole basin of this small tributary, and we got ladders and stripped those trees of all their leaves. And then we introduced the leaves into the stream and tracked what natural leaves did, what leaves in bags, mesh bags is what most everyone was using, and then what leaf packs did. And I thought we showed real conclusively that the brick packs tracked the aerobic natural leaves perfectly. The mesh bags tracked the leaves that got buried perfectly. So if you turn a leaf black, it'll look like the ones in the mesh bags. And they handled, of course, whether they could get into the bags. All that stuff. But then other people started looking at that and the people at Brigham Young actually did a whole series of experiments with cages of different mesh sizes. You know, it was pretty clear that the two things that were driving the overall decomposition of litter were temperature and oxygen in streams. And so if you limited the oxygen in any way, then you excluded the hypomycete fungi. And that was Klug's sort of help. And that got started to be looked at a lot of different places. But just like they're still pulling logs out of streams in Northern California, they're still putting leaves in mesh bags. And so I'm not sure we've exported efficiently. But I think a lot of this exploring this sort of thing, and then whether, exactly what the drivers were that let the chemistry part of the thing. That got exported as well. Particularly through a couple of microbial people at Pitt were working on that. And that led to the—you know, if you get the right lipids and put it on filter paper, the bugs will eat the filter paper. So they're really after lipids. And I thought that kind of approach is that, and again I think there's been some exporting of that, but as I said this morning when Charlie Hall called me up, and talked about should we

reinvestigate some of the caloric stuff. And I said, Well, you know, calories is really all about lipids. That's what the six thousand calories is driven by. How much lipids there are. And so we really need an inventory of lipids, now. Non water-soluble tracers for setting the system up. So I think in the general area of does this recurring system work similarly around the world, it's not the species of the leaf, but there are in fact fast medium and slow ones that are all dependent on cuticle thickness and biochemistry and lipid content, and that sort of thing. And then I think that a number of other labs are certainly, and Suberkroff was with us. And now, and when he left, a lot of that—he fertilized us, and that fertilized the things that he did.

BC: Barnes even brought his cages up into the desert streams.

46:36:

KC: Yeah, right. So there was a lot of that sort of thing going on. So I think that kind of detrital focus and riparian significance was . . . and so, in fact my wife Peggy says that my pictures are boring. Everyplace I go, I pick up litter with some shredders. Always the same hand, this just looks like seeing one leaf—the hand gets older, but it's all the same thing.

VR: What about the functional feeding group concept? That was developed, started a little before the continuum, but it's such a part of the perception that most people have of the continuum. Did that develop much after the studies in the late seventies and early eighties.

47:20

KC: Yeah, again, that was being associated with Klug helped that along a lot, because he was interested in the microbial part of the detrital work, and the experiments that we were doing showed that the fine particle feeding ones were not able to select for quality. Whereas the shredders were definitely able to select for quality. So it was a real sort of strategy shift there once you get to the small particles. You can feed blackfly larvae aluminum particles that are the right size, and they'll eat them happily in a lab, and you can mix sand with detritus in the right sizes and show that the growth is totally controlled by how much sand is in there. There are a lot of things like that, so we were trying to check some of the ideas of the functional group. And that's certainly what I've been working on the last fifteen years, really. And right now we're trying to develop a strategy for basin-wide kind survey of invertebrates. And that's about the time I hooked up with Merritt 'cause he came to Michigan State. In fact, he came in and started doing the aquatic insect stuff that I was doing by commuting from Kellogg up to campus. And so that, the idea that you can continue along the line to get better and better at the "what is it" part of invertebrate work, with just the systematics. But in the meantime, you can do, "What do they do?" parts as they go along with that. And that trend sort of cuts across the "what is it", and Rich and I we'd been working on that together. And one of the ideas of the book was we need to continually track kind of the best ecology information at the generic level for all invertebrates. And that's what all those big tables are about in the

book. But it's never, never quite worked, and for most people it's still what's in the gut contents, and you simply cannot match gut contents with the morphology and behavioral strategies that collect it. So we're still working on that. But now we've—Rich and I, and Peggy's been involved too. We've gone a bunch of different places, in Europe, and in South America and Australia, and tried to see how that system works in different locations. And I think that's the basis for Noel Hyne's statement that you pick up a rock anyplace and you can recognize the bugs. They look the same. There are different families, and maybe different orders. In South America, they're different orders. Or in Australia, there's the stoneflies and the mayfly and so forth. But that's just another signal that the parallel or convergent evolution and morphology of behavior for linking to the resource is pretty universal. So that's really where we've been going. And right now in Northern California, the north coast, we're trying to implant some kind of an invertebrate survey approach where you leave the field with the data. The problem with invertebrates is you leave the field with the bottles, and then it's a thousand years before—And you need to leave the field with the bottles, cause then you can do the rest, if you've got the time, the money, the people. And get at production and other kinds of questions that you might want to ask by sequential samples that you look at in the lab. But you can leave the field with some information. That was the idea. And that tracks real well with people doing fish surveys, cause they leave the field with the data. So now we're trying to couple up the invertebrate and the fish surveys together, cause there's a huge effort to get out the statistical design of how you sample fish populations, juvenile salmonids, and we've just tried to walk right into their design, and say okay, we're going to also look at how the invertebrate communities are set up around these rearing areas for these fish. So that's kind of where, the spin-off from the continuum certainly for us was that. I mean, that's where we really went. And all the people involved helped with that. I mean, Klug for sure, and Bob Peterson in a lot of his work was interested in detritus and partitioning of it, and then with Rich, that's trying to go around the world and look at those systems in other places.

52:11

VR: Thanks very much. Robin, what about at Stroud? What were some of the immediate and maybe a little delayed spin-offs that happened from the continuum work?

RV: Yeah, I think the immediate work was continuing to focus on the benthic communities and their interactions with the input of leaf vegetation; organic material coming in from the water shed. We were looking at a lot of linkages with the floodplains with the hill slopes. We did a lot of manipulation studies on food quality of leaf litter, recognizing the terrific changes in the composition of the eastern deciduous forests with the residual beeches that had low values versus the hardwoods that are more economical to cut and remove for selective use in the change in the composition, and also the fact that we were only dealing with twenty percent forest in a watershed, and eighty percent open land, what was the impact of this kind of configuration on stream communities? We began really early in recognizing that the temperature differences in meadow streams versus woodland streams was very dramatic. I mean, it'd come out of the forest in the summertime, and immediately jump almost immediately ten degrees, goes back into a big

woodlot, cools right back down. So I think in 1973, I began planting trees along stream channels in White Clay. I was kind of naïve at it in the beginning. I called up an old friend from University of Maine who worked for the forest service. He was the only one with reforestation on strip mines and had these super poplar trees that would grow very fast, and I wanted shade quick. Quick results. So I called up Mirslov Troppepovski. He was escaped from Russia, and was a forestry student in Germany and then at University of Maine. And he was enthusiastic. And he couldn't believe the productivity of White Clay Creek soil, floodplain soil versus spoil banks of mining. So we got the trees growing, and shade there, and I think we wanted to look at how efficient insects were in processing various species of leaf litter, in these different temperature zones. And there we came upon the fact that temperature was a major factor in growth of insects, and their fecundity, and those studies went on to be very successful and rewarding. And in another large geographic study. So we were coupling sort of reforestation, what were the best ways to do it, what species of trees would benefit communities, maybe naively. You can never reconstruct a forest to the original, natural state. But it was very important to develop a canopy over these streams. But then again, can you overdo it, because beavers were always in these watersheds, and then essentially got back into White Clay, and I can remember studies as an undergraduate, looking at beaver meadows, and aging shrubs and trees on meadows. So meadows were always a component of streams in the eastern deciduous forest. So maybe it's a mistake to completely reforest the stream, if you don't have the beaver component to make these kinds of shifts and develop meadows. So that's an interesting aside anyhow, but I think my work led more back to the benthic communities, and the nutrition, production, how food was a major component, of where it came from, from the detritus, from outside production. So we still had a very integrative team to look at these questions. And I was also very interested in how water got into streams. And this is particularly important I think in these forested streams in the east because all the uplands are well developed into agriculture, either pasture or farmland, and the runoff from those areas are very severe in the non-crop season. And water gets shunted into these woods. You get terrific erosion. The forest floor isn't well-protected. And now maybe with this earthworm work, we know we have less protection, it's going to be more severe. At that point, we brought Denis Newbold in. He became available, and I had money, and the position came. So we could establish Denis at the lab. So I think that was really a major addition to the capabilities.

58:17

WM: Yeah because in part, Dennis brought the spiral home and the capabilities he had.

VR: And he was a Berkeley product.

WM: He'd looked in logging impacts as part of his doctoral work.

BC: Obviously Dennis has had a lot of impact because I'd like to throw in just a little bit more about—we mentioned earlier about the FPOM transport. I can remember Wayne and I starting out on this project and we're over at his house for a week or so to write that first proposal. We were writing, showing these little simple curves, and we'd put on

these radio tag particles here, they'll go here, and then we'll know a lot. And we started out pretty simplistically, but boy by bringing on Dennis and Steve Thomas, and their modeling capabilities. It just took us—I hate to use this term—to a whole new level, cause I never figured out how many levels there are you can get to. But anyway, these guys really took what we had originally decided to try to do to a level that frankly, I don't know if we'd made it without.

59:28

WM: Yeah, I honestly, it was a great coup that we acquired Denis, because we acquired the modeling mathematical capability, and we were able to marry the spiraling concept with the river continuum concepts and move both very forward. And so it was really great in that regard. So there's a tie with Denis. Not only the spiraling ideas, but then with the Stroud lab. And we've been continuing our collaboration with the Stroud lab on that route and through Jim Brock's connections with the metabolism aspects up until the present time.

60:05

RV: I think it's interesting how the loop from the spiraling concept goes all the way back to Bob Ball and Frank Hooper who originally designed in the late fifties, the introduction of P-32 into Titchin river stream. That was a terrific data set that really never made it into publication, and I guess the data set Bob Ball took it down and gave it to Denis at Oak Ridge, I believe when he retired or went to the Water Institute, when Ball went to the Water Institute on campus.

BC: But that transfer, one of the major interesting things about that whole thing that was almost an aside was that they found the P-32 upstream. And that was the first time we started looking at a little bit of migration upstream. Everybody thought put it in here, it's all going down here. Instead they found it in bugs upstream.

61:13

WM; Well they found it in the treetops, too, so I mean it was a real linkage with the riparian and terrestrial that hadn't been appreciated fully. In fact, I think maybe they didn't appreciate it. They were afraid of the contamination issue rather than that—

JS: They never did that experiment again.

VR: Well we related if we remember the Doe Run spikes that Krumholz would do when it was right above the Heaven Hill Bourbon intake, which I've never drank Heaven Hill Bourbon since.

WM: Well, that's what made it heavenly.

61:53

VR: Robin, any . . .

RV: Well I think that's about all of the major things that I've done afterwards.

VR: Jim

JS: Well, my tack was very different. These guys are hard-core scientists. And my interests turned to bigger rivers, and the issue then, to me was, if all the little streams are dominated by forest—cause that's the reference I was using, a forested system, or some sort of vegetation system, the assumption was that the big rivers were less influenced by that. And when you started to go through the archival records, there was some incredible interaction from all these tributaries, all these upstreams, that while there wasn't necessarily an energy driver in the big rivers from that, but there was a habitat, an artifact, and a floodplain that was very much dominated by both upstream and adjacent vegetation on the large wood. And so I started to go back to the old Army Corp of Engineer Records and pull out, snag records, and again it was aimed where they said it's only happened in the northwest where we had great big trees and it was obvious. It was obvious whether you looked any place in the country. Or even in Europe. And then that got into looking at, well how have those rivers changed so that our perception of the world just doesn't match what it might have involved in the Wilamette paper, and then moved on to something else. I think at that time, then St. Helen's blew. And I was interested in beavers also. I think Amy Ward and Cliff Dahm were very influential. When St Helen's blew, those lakes they said they're all dead. Those lakes were more alive, and the streams were more alive than they'd ever been. It was just billions of anaerobic bacteria. And so I started to look at that. And we were starting to look at these stair steps through log jams and through beaver ponds. And then the chemical changes and the release of N and P and Cliff went on and did a lot of that in New Mexico. It was using those same kinds of techniques. Of looking how essentially a forest or a vegetative kind of step then created whole different nutrient pathways. And indeed we found our fish were bigger there, and more action was going on just below those. But I never really got back. I got more then into taking the vegetation control and applying that. So my career path took an archival bent and then also started to look more and more at how do you translate these concepts or what we know about influence of forests on streams into how we might want to manage them if in fact we wanted to maintain the salmon and other things. But again, you're drawing on this huge group of people. Ken came to Oregon State, and then people that weren't associated with it, like Bilby and Bisson become important colleagues that push forward a lot of thinking about that. Stan Gregory is now into big rivers, in the Hilamette and Wilamette Outlet. So I think that there was an evolution where we grew up in very little streams and then we started to look at it a little differently. And right after, cause I pulled away from the continuum for personal reasons and family reasons in seventy-seven, and went to work for Weyerhaeuser, and at the time Dick Waring at Oregon State and Jerry Franklin recommended I get on a National Science Foundation panel. So I became a part of the ecosystems panel, the ecology panel, just when they were splitting it between ecology and ecosystems. And intellectually, that kept me alive, and that allowed me to advocate

for things like Jeff Ritchie's Amazon proposal that got started, where again you're looking at a size and scale that we never even thought about when we were out on the Salmon or even looking at the Wilamette next to Oregon State. So in many ways those have come home to roost now twenty years later, with all the stuff that Ritchie's group, and Milac and others have done. And so I feel like I had a part in moving that around. But I think the big deal is still the influence of forest on rivers.

67:05

KC: And a lot of us would attribute you for the historical perspective. I mean, we looked to you. It might have been focused on wood, which was a logical thing

VR: And that's so that you could get some data. You liked data.

KC: We brought you into the Kissimmee basin, and it's quite clear that there was significant cypress in the Kissimmee basin. And cypress, of course, is no part of the restoration, whatsoever.

JS: Right, there are live oaks on the levy.

KC: Yeah, right. So there was a definite big wood story there. In fact, it probably set up the braids to begin with in the river. So that kind of historical perspective which you can tease out of the history of the logs actually was really pretty important, I think.

WM: Our whole conceptualization of how those large rivers looked is influenced by the fact that we thought here and now is what they looked like before. And Jim's given us the insight that they could have been looking very much different.

KC: And they're not planting any cypress trees as a part of the restoration and that's stupid.

BC: Did you leave, or did we throw you out

WM: Well one of the things I just sort of added, is one of the things that you're seeing here is this new opportunities that emerged for me and some sort of life changes that occurred. So there really wasn't the coalescence for going on for additional funding to continue the river continuum. I think that's really important. Bert will probably talk a little bit later about his favorite part, the Middle Fork. But in part, our work on the Middle Fork put us in a position that in 1979, in this same era, there was a major fire on the Middle Fork. And that changed the direction of my research. Using a lot of the tools from the River Continuum, but applying them to long-term effects of fire, initially in the Mortar Creek section on the Middle Fork of the Salmon, later in the Big Creek section, which we sorta had just gone by and were awed at when we passed by it, and incidentally, the Big Creek section is really what I think formulated our ideas about the impact of tributaries and got us thinking and questioning, and developing some thoughts along those lines. And that's reemerged recently now in a whole body of theory that's

taken precedence in the literature. But it really had its original ideas about influence on continuum ideas back then. Anyway, Big Creek was another place where we had fires. Simultaneously with that, Yellowstone erupted and we were invited to come in and do the research on that, and that's extended into a real long-term data set now.

69:55

VR: Let's talk about the Middle Fork studies, and the float trip That was done in the area. Burt, could you put it into time context, too

BC: Yeah, this was in the summer of seventy-eight, and I guess one of the reasons that I like to talk about this is because to me this was still one of the best trips for just stimulation and exchange. I had a ball on this trip. Despite Ken and I getting thrown out of the raft, sunburnt feet, and the things that went with it, it was just a great experience at looking at all these aspects with all these guys, and I just think it was a tremendous trip.

70:33

RV: There was a lot of hard work, and we learned an awful lot about what's on the bottom of the river, and we did a lot of sampling.

WM: We came up with a variety of things about rugosity on the rocks, you know, whether they were smooth or rough, microhabitats, kind of distributional things, diversity responses. So there was a lot of invertebrate sampling, a lot of on-stream processing. Just a great effort in that regard.

BC: We saw a lot of those things we looked for in the continuum site. And we also did analogs. Of course, we couldn't carry the chambers. So I did periphyton and chlorophyll analysis as sort of a surrogate to those in all the different places we stopped. I just think it was one great trip and—

71:24

VR: How long did it last?

BC: Ten days. I remember.

VR: And the five of you went?

BC: Uh, you didn't go . . . Bruce Wallace came with us as a guest. And we started at what Dagger Falls? Went to Indian Creek, sampled in Indian Creek and above and below, same with Loon Creek and Big Creek.

71:44

WM: We were looking at tributary effects.

BC: Yeah, we were looking at what it would be like above where the tributary came in, below, how far the effects go. It was just a real stimulating trip.

KC: We learned how to fly fish when there's stable flies around. I got into those things. And that's a biting insect that's usually associated with agriculture. And this is a pristine river, was full of stomoxies, which breed in detritus. Nasty flies.

BC: To me, it was one of the real highlights. It really was.

WM: And besides us, there were a number of other folks that had been on the technical crews or the research crews or the graduate students. So it was a mixture.

BC: Plus we had a couple of professional guys to do all the cooking and all this, to take care of the amenities so we could work. And it just was a great trip.

72:53

KC: And we got the garbage collectors to collect samples for us. Later on . . . Forest guys would pick up samples for us, cause they were making this run repeatedly.

WM: As an aside, we were caught in a rainstorm at Loon Creek, and it was my first experience of somebody getting near hypothermia, because Robin and Dave Funk I think were sampling in Loon Creek in this downpour and they just kept on sampling. They would not come back. And when they came back, they were somewhat incoherent and silly. You know. But it was recognized, we sort of pulled out, we got a fire going, got warm things into everybody, but a real scary thing on our . . .

BC: To be on, you go out, sample at your place, you come back to the lab. Here you sample, you learn about how to preserve them, at Loon Creek. A plane flies in, brings supplies, flies samples out. This is a whole new game. It was really an experience: Until that last day. We could do without that last day, though, couldn't we?

VR: One of the things I'd like to ask you to all talk about is twenty-five years later, after the 1980 paper came out, there's still some misinformation or misconceptions about what the continuum said. Actually, didn't say, but has been presumed to say. Could we talk about that just for a few minutes and just set the record straight that these things are really still misinterpreted about what the RCC proposes?

74:47

MF: Just a second, let me just make a quick change of tape.

VR: Is that a time because you don't want to talk about this?

WM: No. Because we're out of tape.

KC: I'm not sure I have anything more to add.

RV: Yeah, I'm not sure where we're headed on this.

WM: Well yeah, I thought we covered it. I'm not sure that more and more ground could be gone through. I think the biggest problem was the zonation. The assumption that we had said, by god it's absolute, you know, we'd really intended this sliding scale.

VR: Okay, if you could just do that and then we'll run with that.

JS: Yeah. To me the amazing thing is how literal the science community is.

VR: That's fine. Say that.

Group Meeting 6

VR: So I'd like to ask you again about the misconceptions that are still floating around about the continuum.

WM: Well let me take a shot at it just because it's a reiteration of something that I said earlier. I thought a couple of key areas are that . . . One, a lot of people continue to think of this as a zonation thing. They don't pick up on the implications in the term "continuum". The gradual, progressive changes that take place. So you don't expect real sharp changes. You expect real gradations in these changes. But we had to illustrate it—we felt we did anyway—in the original river continuum by breaking it into points and rather than talking about every infinitesimal point along that line, we took two or three or four key areas and described those in detail. So I think people have misconstrued that into thinking, "Oh, those are zones." And those are in the classic European sense rather than in this whole new paradigm of gradual progressive changes. A second is the assumption that we thought that all streams started in deciduous forests, and we didn't. We considered that very much in detail, in part because I was coming from a desert biome, and I had another perspective of the whole distive system. But again, to avoid confusion, we started with what we knew the best. We'd all worked in deciduous forest streams, and we could describe those in some detail, and so we worked with that. But the overarching issues are, "What are the drivers" Whether it's allocthonous detritus or whether it's sunlight. And then depending on the proportion of those along the system, that's the way it plays out within the system. And I think that's been missed by a lot of people as well, and we tried to correct that with the subsequent publication. Another is the pristine nature of the model. In essence, for the modern system, we were erecting a null hypothesis against which you'd test these things and say, "Well this varies." And then we say, "Well, what's the reference point? What does it vary from?" It varies from this model that we set up and which we tested and basically conceptualized from the standpoint of a pristine system. And then ultimately emerging later as we got into this were other issues. As we had the data and the insight, we could say, "Well, look. There's more impact from tributaries than we'd originally anticipated. There's more impact from local geomorphology than we'd normally anticipated." And all those things could easily be put into that broad template once we had this model that was the river continuum.

2:55

VR: Thank you. Anybody else have any—

BC: I'd like to emphasize the one point he talked about. There is a model starting in the deciduous forest. Cause again, that's what we knew best. But to this day—and I probably milked this as much as anybody giving all these talks to trout clubs and things now—you really have to emphasize that the original model wasn't built on the streams, like in Yellowstone we're going to go out and sample when I teach up there. Things like this. The model wasn't—People still take wherever they are and try to cram their model into it, without realizing these historical perspectives

3:38

RV: I don't let this deciduous or coniferous forest bother me one bit. It provides inputs. And that's what we were dealing with. We were dealing with a partitioning of the forest as the stream enlarging from the inputs of tributaries or groundwater along its reach. So I just don't understand this hang-up on it was developed in Eastern deciduous forests. We had the data from there, from Eastern deciduous forests. That's where we lived. That's where we worked. But it's not the significant issue. This concept was designed and applies mostly to eastern deciduous forests, when in fact it doesn't matter. The rate at which the stream partitions and lets light in. That may function by the type of forest it's in, whether it's constricted in a canyon. Those are factors, too. But how wide the flood plains are. These are differences in topography. They can all be accommodated. So many of these side issues are just shoved at the back of somebody looking ahead at a new design, a new framework.

5:03

BC: The point is, we recognized this. We even made trajectories that said P/R ratios is this—

RV: So I let that stuff run off. And I think that some of the more interesting aspects of the research that we put forth for people to look at and evaluate was that stream communities are structured by local inputs and what's transported downstream. And what's very interesting, at least to me, is that the communities that are structured in these reaches along this continuum exploit these two resources, and the inefficiencies are the transport downstream, so that when you look at a progression of these communities' structure along the continuum, it becomes a more efficient-looking system. And it gives a terrific framework for pollution ecologists to look at holes in this community structure and leakage. So here's something that's out there for everybody.

6:22

JS: Yeah. It's a world view. The power of the river continuum, in my mind, is a heuristic device. Something that you can talk to people about. How you might think a stream would operate that they can understand. And what has amazed me in looking at papers since is the literalness of scientists, of taking a little piece of a point in it, when in fact to me, the reason it probably gets continually cited is it's a good story. It's a world view. It's a stab at taking all the little reach stuff, all the little rock stuff, its own portion, and talking about this progression of small to large. And I don't think we had that kind of framework. And we're still struggling. We've done very elegantly in taking parts of big rivers and flood plain rivers and progressed that way. But for an overall, looking at little streams to big streams, I haven't seen as interesting an explanation and as easily talked to students about as what that was.

7:36

WM: I think one of our innovations was that however this template changes, the biota will respond, and that response is predictable. And that's really the key. If you mix it up and change the template, then you ought to expect some sort of biotic response and that ought to be able to be described and predicted ahead of time.

7:57

KC: You know I think the current attempt, I won't say discredit the continuum, but to dissect it, is that the pattern that's really out there is the pattern of the discontinuities, not the longitudinal transport. And Lee Benda is on this. And that's a strictly geomorphic argument at the moment. There's basically no biology whatsoever to support that. And they're looking for it. And that's fine. But this is the kind of thing that happens. Perhaps the people that objected to it most violently for whatever reason, have spun off into a whole bunch of new experiments and things. I mean, that can't do anything but help.

8:37

WM: But again, the geomorphic argument is one of hot-spots. Whether landslides come in, or whether you have floodplain openings and side channels, or tributaries that came in and you get a very complex channel, and set of channels. And it's real. I mean, there's no question it's real. But you don't hang a river system on the nodes. I mean, they're an important part of it, and in fact what we were dealing with was that whole continuum transportation.

JS: In fact I've argued that it's a different scale. We were looking at a different scale. And scalar differences are really important in terms of what you see and what are the drivers. So the local tributary scale is a lot different than the whole watershed, the whole basin scale that we were viewing.

9:34

BC: But how would we argue with government that biology doesn't track the geomorphology. We never—there was a whole idea. And so yeah, if these are geomorphic hotspots from a geomorphic perspective, does the biology track that. I think there are some overriding things that kind of smooth that out for the bigger picture.

10:05

RV: You've really got to hark back to the fact that streams exist for two reasons: to transport water and sediment. And the biology has to fit in. And the sediment delivery changes because of loss of vegetative control, there are going to be changes and the biology will shift. So I don't know where all the arguments are coming from, but they're healthy.

JS: And again, I think that you're talking about trying to redefine a new world view. And I really think that to me that's what I look at, is we took a shot at a world view. And it hit. Why would everyone bother going back to it either to dissect it or to apply it. It was a world view. And we'd expect some other group of people to refine a world view and come up with something in the future.

11:11

JS: And in the competition for funding, you don't get funding for redoing the same old thing. You have to have a catchy name and a supposedly new concept.

KC: One thing Jim mentioned that reminded me of something is that there's no fish story in here. I mean, we didn't spend—at sites, at our own sites . . . But there's really no fish story. And I think that's kind of unique, because a lot of these things, in Europe especially, have been driven by the fish story. "Here's a fish story, and now we'll try to fit anything to the fish story."

RV: We entertained the fish story. I think we wanted a fish story, but we couldn't hook Jim Hall.

JS: He looked at the immensity of the task, particularly where we got to the large river, and he said, whoa.

KC: But we didn't do a lot of fish measurement for the continuum. Bert and I did a lot, but that was very few species.

RV: A detailed study of the fish population would have been comparable to trying to do something with the benthic community and a production estimate capacity, and that would have been an expensive project on its own. So unfortunately the fish and the benthic productivity had to be sacrificed in this study.

12:31

KC: There're no species of fish out here compared to the East anyway, except we got them in by...

WM: Well and the Salmon, operationally, we were constrained with the fact that we were going with a system that had threatened salmon in it. And Fish and Game was not keen on issuing collecting permits. That even got worse as—

VR: He's interested in the ocean, not . . .

KC: But since fish drive so many studies around the nation, people are trying to see how they'd link into the . . .

13:04

VR: In a way, it's amazing you got so much money without doing fish.

KC: Well we got it from very non-fishy people.

JS: Actually, at the time, the National Science Foundation, unless you were Don Hall's Ponds, they weren't as keen on fish as a model to get at the ecological stuff. And some of it I think was on the heels of the IBP where they were more into trophics and energetics and the nutrient cycle. They seemed to be the hot—and there were technologies, certainly with the isotopes, that were breaking out of the national labs and use

WM: So there's a current paper, recently published, by Kurt Fausch and a number of co-authors that directly gets at some of these issues, and expands to really emphasize the fish work and the linkages and to look at the system in terms of networking as opposed to the linear model that we worked with, in part as explained earlier, because of financial constraints.

14:10

VR: What I'd like to do is let's go around the room and ask of the tens of papers that were produced directly from the river continuum work, which one do you think, personally, is the one that is your favorite or has the most telling message about the overall project. Jim, can we start with you

JS: Oh, the first one. No question about it. It was the story.

VR: The 1980 paper, Canadian Journal of Aquatic Science.

JS: And so, that's one I go back to. The others give some life to it. The monograph paper and all are really the hard numbers behind it. Then, I don't know all these thirty-three papers. I don't remember them all. But those were real important. And in fact I think the monograph paper was—

VR: The 1983.

JS: Yeah. To me the 1983, the inter-biome one, really set it. And I remember those much more than the ones we did at our site. I mean the sites were interesting. But that wasn't—to me, if I had to give people something, I'd give them those two papers.

15:35

VR: Bert, what do you think

BC: I can't argue with that. The inter-biome paper, after the first one. The first one set the stage for all the others.

WM: The first paper gave us the framework. The inter-biome gave us the meat; the flesh onto the frame.

VR: Robin, do you agree?

RV: Yeah, I certainly agree. And I think the cross-biome paper is the important meat to pull it all together, and I think Tom's primary production paper was a very good—

VR: That was in the . . .

BC: The one that summarized the . . .

WM: Basically inter-biome metabolism comparisons.

RV: So I think we had three major . . . Ken, Wayne?

WM: Well, first of all, those set well above, so I fully agree. I think for me, the most fun, in a way, and the one that was very important, was the Recent Advances in Stream Ecosystem Dynamics, where we were able to sort of add our additional modifications. The insights we gained. And it was intended as an answer to some of the critics. The Winterbourne and Rounick paper, and to the Statzner and Higler paper. So this was kind of a chance to catch your breath and say, "Look, you guys are really taking this in a wrong light. Plus, we recognize that there's these additional things that need to be cleaned up. The sliding scale idea, the tributary effects of the Sansaw. So that one I would add as another one that I—

VR: This is in the Fontaine and Bartell's book?

WM: No, no. This is Canadian Journal. The second paper in Canadian Journal of Aquatic sciences.

17:20

KC: I'd agree entirely with the three that came out. The first one, clearly, and the inter-biome was—Putting that together was where I felt we really understood something about how well the story actually worked. And then Tom's work with comparing the—and the chamber technology was so important. And measuring primary production, the PR ratio measurements in the field were so important. To summarize that on an inter-biome basis was really terrific, I thought. I enjoyed a lot, I don't know if you could get a number, the organic budget paper we did? Now that was a lot of fun. Stu Fisher was involved in that one.

VR: Where did that appear?

WM: In the book. In the stream-dynamics Barnes and—

KC: Oh it is? Okay, it is. Yeah, that's right. It's in that book. Bout the only place we could . . . And that was a lot of fun, because we talked a lot about scale. Full scales, and capture of flood plains, and all that kind of stuff, and we went around a lot with Stu, 'cause we were trying to match up these budget papers that had been done every place, and they didn't match worth diddle. And trying to make sense of that. When we all sat around with Richey and tried to figure out where the organics were coming from in the Amazon, the lower Amazon, and everyone said, "Well it's all the floodplain story." And so we did the numbers, the best numbers they had, and it didn't account for a major portion of the organics.

18:55

JS: This was at a meeting where on the back of an envelope we were estimating the transport of carbon from the rivers of the world into the ocean. And we had Tom John and Ritchie so we were using big numbers. And then, the amazing thing about dissolved organic carbon, as opposed to the particulates, is that the range is pretty damn narrow. I mean, what, a couple to a half or something. So you could then get away with just multiplying a single one by something.

19:30

KC: But then Ellis said whole damn islands are coming down as—

JS: Right, but the tributaries were huge, and . . .

KC: Yeah, they could not escape the fact that the upstream delivery was important. They tried hard to say it's just this kind of story, but it just doesn't wash.

WM: I'd add maybe one other paper that for me was perhaps the hardest one to do, and yet probably equal in importance to the inter-biome comparison paper. And that was the eighth order Salmon paper, because that really brought together the second part of our proposal. And it's a major publication effort in that regard.

VR: And that appeared in?

WM: Oh, where did it appear? I'm not sure. It was such a relief to get rid of it, I can't remember where we went.

BC: I see it with a blue cover. And that's ecology or ecological monograms.

WM: At any rate, it took a lot of time just because the funding had ended, and a lot of interest of the other PI's and so on were going other directions, and that just drug out over a number of different Christmas breaks, where I and Bob Petersen spent a lot of time—

BC: You can see the care with which we selected the journals to submit our papers. We have no clue. How fast will it come out? How quickly we get a review?

21:09

VR: Well I'd just like to ask you if you have any last comments. What to you if you have a comment that can encapsulate the idea of the river continuum or your ideas about it, what's a closing message that you'd just like to leave? Robin, do you have a . . .

RV: I think that it's really exciting that a group of people could get together, flush out an idea, design some experiments and hypotheses to test, and get an interesting job done. It was diverse people, and Wayne did just a terrific amount of work, and he was—he was a general.

KC: You know, I think about my dad, just before he died, sorta said, in context of all the fishing he'd done, said, "I had a hell of a run." We had a hell of a run. We really did, you know? It was just great.

JS: Well I guess—yeah. Cause I came from a liberal arts background. I was a philosophy/ political science major. And then came and got influenced in biology and got a masters degree in biology. And to me it was always just data collecting. And that was pretty boring to me. Really is. And here was a place where you could see that science was about creativity, and about creating this story or world view. And that was just was a total turn-on. An adrenaline rush of trying to see what it was and then figuring how you could measure it. Even though I didn't like to measure it. But at least I had a framework. I had a story that was fun and it was exciting and it was evolving. You needed new technologies to get into it. You had a great group of people to interact with. And again, you learned a whole bunch about interacting with people. The good, the bad, and the ugly.

BC: I might make a couple observations I got out with. First, to me, it largely put to rest this idea of trying to classify rivers, and the businesses of zonation and all this stuff. That it showed the downstream linkages. And the other thing that surprised me, in a way, was how quickly the concept was not only accepted, but criticized. And a hypothesis is for long-term testing. But it didn't take long to get some critics out of the woodwork. And this surprised me a little bit.

RV: Instantly.

BC: Yeah, instantly.

VR: Which probably said it was a good idea.

WM: I would just add that I think that's the way science is supposed to work. And sometimes we get a little thin-skinned about it. But basically skepticism is healthy.

BC: Sure it is. That's what a hypothesis is for.

JS: That's what we do best, is falsify. That's the only thing with certainty we can do.

24:24

VR: Wayne, do you have any final thoughts?

WM: No, I—well, of course I do. And some I'll try and restate here. One observation that kind of goes along with what Robin was saying, that I kind of had this conception that to do research, it sort of had to hurt. You just really had to work yourself to the bone, and you had to be really narrow and stringent about it. And Robin comments to me at one point, that if we're going to do research, it ought to be fun. And I think that these people have just reflected in their comments that it had been fun. And the fun, sometimes, is really hard work anyway, but it's the intellectual stimulation that gives us fun, and the sparks and the overall reason for wanting to go on. And then the ability to actually do something about that; to test those things and to bring a whole bunch of other great people together. It's not just the PI's. It's this whole sort of army of folks that contributed. Those are the lasting things that we have. And overall, I've been gratified. And I think that the model that we've evolved, and that we've put forth has held to the test of time and have changed the direction of science in stream ecology profoundly and in a very positive way. I think we're better off and just as excited now as we had been twenty-five, thirty years ago.

RV: I think a little add-on to that is that there were so many people who helped us. So many young people that helped us. And I think just seeing their growth and going on into science and things that they're doing. I think it's very rewarding. I take a lot of pleasure in seeing the science that young people are doing that worked with us. And that's sort of a continuation of your life into something else. I think a second aspect of our interpersonal is that we really made good friends of one another. I mean I just love to go visit with Jim and hack around the woods, travel with Fred Swanson in the woods and rolling rocks, and discussing landscapes. Continues today as a pleasure. Going to Wayne's and hiking around in the parks and landscapes with Wayne. It's just a continuation of friendship that's wonderful.

27:07

KC: It's sad that Bob Peterson ended so soon. He died way too soon, and so some of these tendrils we've sent out got chopped off way too soon. But yeah, Robin's right. We became good friends. It's really fun. So still, Robin and Wayne have been hacking around in the forest fires. And it was great.

WM: Yeah, so I mean, this is fun, what I do. So this is. But what a great thing to get back together again, and kind of have this opportunity.

BC: And there're some aspects that we didn't cover, like visiting Wayne, and getting put up in a trailer sleeping over a skunk. Some of these things, you know, they were still great.

WM: Well since you didn't bring it up, I won't say what I was thinking about that.

VR: Well let me say one last thing. From the time this idea started, all you kept on asking me, "Why are you doing this?" And I think I can sum it up best by telling you, when I finished graduate school and started thirty-two years ago, the five of you were the film stars. You were the celebrities. And my entire career, I've seen the influence that this has had in terms of putting people down paths that really sidelined them because they got caught in the details, or of just turning people on to the way rivers work. And the reason that I wanted to do this is I think this is the great story in aquatic ecology for the twentieth century. And I really wanted you guys to be able to tell it. I have to say this has been one of the best two days I've had in my life in terms of just stimulation. And let's end with a big thank you to the five of you. Okay. The film stars. That's the only term I could use!

Ken Cummins Interview

Vince Resh: OK. Ken, thanks for coming and talking to us. This is your chance to tell your story of the RCC. And I'd like to start by asking you...tell us about your early life. What made you become a stream ecologist?

Ken Cummins: Um...I grew up in Chicago, and escaped to Wisconsin as frequently as we could, and so I became an early fisherman. I was interested in streams from the beginning (you know, stream fishing), but I think the turning point was I went to a small college in Wisconsin called Lawrence University. I was headed pre-med, like a lot...I think most of the biology people there were pre-med, and I took a course from an ecologist named Garth Kennington who was on the biology...there were only two biology faculty, and he was a natural history type ecologist. He just completely turned me around. In one term I went from wanting to specialize in diseases of the rich to actually becoming an ecologist, and he was really the motivator for that. Wonderful man, just devoted to this small college teaching profession, but had his own research program that he did in the summer in Wyoming. So, that kind of turned me around to ecology, and I had this orientation towards aquatics. Then I went to the University of Michigan for graduate school, and started out in fisheries with Karl Lager because I figured fish and water, that's where you needed to go. So, I took my masters with Karl, and during that time, I took a limnology course from David Chandler, who was a student of Paul Welch's, and George Lauff was a lab assistant for that class, and he was the lab instructor. So, during that class, I got switched over to a sort of system approach rather than just looking at fish, and I became interested in aquatic invertebrates, which is what George Lauff was interested in. So, then I switched to limnology, and took a PhD with Lauff and Chandler, so it was kind of a movement from interested in fishing, then interested in ecology, putting that all together, thinking fish was the focus, and then eventually believing that ecology was really the focus. So, the University of Michigan in that era was a wonderful place to be in graduate school. Slobotkin was there, Harston was there, Smith was there, Fran Evans was there, Bailey and Miller and Lagler in Fisheries. I mean, they had a tremendous talent pool. If you were interested in aquatics, or in ecology in general, that was a really exciting place to be. So, just a circumstance thing, you know. So, when I came out of that program, then I was thoroughly immersed in aquatics, with a systems approach, but a focus on invertebrates.

VR (3:35): You really had a great experience in a different part of the country. How was where you worked, how did that influence the way you viewed river ecology?

KC: Oh, tremendously. From at the University of Michigan, it was the Great Lakes Basin, and the sort of geology was all glacial determined, and so, that was kind of the sort of thing we thought about. My parents had a summer place near Montana. Since I was a kid, we still have it. And so, I would always say Michigan and Montana were the two parts of the world I knew about early on. And so I sort of had this Eastern, Midwestern orientation, but a desire to get west, because I only went out West in the summer. But, I think it seemed to me early on that the thing that really got me excited was setting up in a new location long enough to get some kind of a feeling for the kinds of systems that were

there, and so the Midwest and far West, and then the inner mountain area around Montana, and then eventually Florida and the East coast and Maryland. And beyond that, I've tried all my life to try to go to as many other places as I can. My wife Peggy kids me all the time because every place I go, I find a stream, pick up some litter, and hold it up and show the shredders. I've got a thousand pictures of my hand with a leaf and shredders. She says "They all look alike," and I say "Well, that's the point." You know? They're all alike. I remember Noel Hines told me, he said "The thing about streams that's so great is that you can turn a rock over in any stream in the world, and they do look alike." They're different taxonomically, but they're all the same. And I think that was...Noel had a big influence on me, for sure, from the very beginning. But that idea that you can turn over a rock any place, and you recognize those bugs, because the morphology and the behaviors adapted the same way. It's a different taxonomy. Our key works fine in Australia. You can key thing out. They're all in the wrong family, the wrong order even, but the morphology's there. I just got fascinated with the idea that in some way, every stream is different, but in some way, every stream's the same. And the space in between is what's really interesting.

VR (6:03): I would like to ask you, how did you first get to know Noel Hines?

KC: Uh...at USIL through the International Limnological Society. Um...and first through SL getting to know Art Hassler and Dave Chandler, and they knew Noel and introduced me to Noel as a grad student late on and early professional, and I got involved in the onchocerciasis program that you've been involved in. Noel, and at that point I had served on some of the committees of Noel's grad students, Kaushik for one, who was...they were interested in this leaf litter shredder system, and so I spent a lot of time in Waterloo working with his students, and I got to know him pretty well. And so, he called me up one day and he said "Hey Ken, you kept telling me" I kept telling him "Noel, if you get any chances for me to go any place in the world and see other streams, you let me know," so he calls up and he said "Well, there's this program in Africa I've been advising on control of river blindness, and it's eleven countries, and the black fly carries a disease in the bigger rivers, but when you look at all the tributary systems and everything, it's a great opportunity." And I said "Yeah, that sounds really good, but Noel, I don't know anything about black flies." And he said "What are they?" (*laughs*) He said "You don't go there to look at black flies, you go there to look at, you know, the ecology of the system." He's been real, he was, he still is, supportive of it all. And, you know, he's probably the best stream ecologist we ever had, ever. So, that was pretty important.

VR (7:50): Thank you. How did you first get involved? You individually first get involved in the River Continuum?

KC: Well, we had a symposium the triple A S symposium in 1973, and at that point I was interested, certainly as a stream ecologist, and I had some glimmers, particularly talking to people. We talked this morning about talking to Herb Ross, and people who had a bigger system view, you know, we were all so compartmentalized, but there were people who were trying to put some big pictures together, and Herb was certainly one of

these. And so, in talking to some of the people at Michigan State, Bob Ball, who was Robin's major professor, and some others saying "Well, you know, put a symposium together and try to bring some disparate views about stream ecology together." And to me, then, I was still thinking biology. I wasn't thinking geomorphology, for sure. So, I just went from, as you pointed out, I went from the literature I knew, and said "Well, here's somebody who should come, and here's somebody who should come, and so, that's how I really got involved in it. And actually, with the people, they had a much broader perspective of how streams are the same and are different. And then it was Robin and the geomorphic setting that really got us thinking about this changing from headwaters to mouth kind of approach. I think that early symposium was my sort of card into the game.

VR (9:22): Did you organize that symposium?

KC: Yeah. I set that up. But, I was encouraged to do that by others. It wasn't initially my idea to do it. But, it turned out great. I mean, we had a good crowd there, and a lot of lively questions and things. And then afterwards, we sat and talked a little bit, you know, and I knew right away that these were people that I wanted to have interactions with because they had tremendous ideas and they were all fired up. And there weren't that many stream ecologists around back then. It was a much smaller playing field. And so it just kind of grew from there.

VR (10:00): When we consider the different members of the group, the five of you, what do you think their different contributions were? I'll just go in order. Robin.

KC: Well, Robin had the basic kernel of the idea, initially. And that came from his exposure to geomorphologists, really, and to Luna Leopold. But Robin is always a big concept person anyway. I mean, he's a conceptual thinker, and so he had a general idea, and I think that's been, maybe, his major role. He's a very cautious person, and very conservative in terms of science, and so he was always reticent to publish something, because it wasn't quite ready. So, because of the pressures of the way things worked, we had to always push him to, you know, "Let's get it out. Let's get it out." But certainly, he's the big concept person. Always thinking. His mind never stops. He's always got some new idea. I remember we had one whole meeting where we talked about kinematic waves. He just had these ideas that needed to be implanted into the biological field. He's really good at that. He's been very good at that.

VR (11:25): What about Bert Cushing?

KC: Well, Cushing is the people person. He's the one who was able to keep everybody together. He still does that, you know? He's the communicator. He talks to everybody, keeps us talking to each other. He's certainly technically very competent, and his real interest has been in sort of the primary production nutrient cycling part of things, but he was the communicator. He's the one who would mediate all the frictions and was very good at that, still is very good at that. He's just a good people person. And I don't think we could have gotten along without him. Whenever there was a little bit of an edge, and

some of those edges were good, because they were conflicting ideas that are rubbing against each other, and what comes out there is some kind of integrated thing. He was very good at making that happen. He deserves a lot of credit for that, because I don't...we would have blown apart a lot of times without his help.

VR (12:30): How about Jim Sedell?

KC: This is the real free-thinker in the group. I mean, he's just...I mean, Robin's mind's going around very constantly, but it's more confined. Jim's is just...everywhere. The story we all tell about Jim is being able to give a seminar with someone else's slides, because, he can look at something, and he can build a story. He's the storyteller. There's no question he's the storyteller in the group. And really, if there isn't a story, he's not really excited about it. But once he gets a story, then he'll incorporate everything. I mean, he's terrific. He's just a...and his energy. You know, you've got a people person and you've got a theory person, and you've got energy. I mean, you plug him in a room, it's energized. And his...in the field..."Go you nuggets! Go you nuggets!" He was always there firing us up. He's been really, really important. And when you can think about what he did, he's the most junior in this bunch. When I went to Oregon State, he was still just finishing up, pretty much, and I was on a post-doc, and so he came up to speed fast, and actually went out in front of us. There's no question about it. He's been terrific.

VR (13:49): This is just an aside, but having been your student, was there a transition in how the two of you dealt with each other?

KC: You know, I think it would have been real different if he had stayed with me. What happened was, when I got the chance to go to Michigan State from Pitt, I asked all my people "Do you want to come with me?" And Bob Peterson did come with me, because he had just started. But Jim was quite a ways along, and one of my former grad students, Bill Coffman, was at Pitt and agreed to come in and basically teach my courses, since I was leaving. And so, Jim switched to Bill Coffman, he's a grad student of Bill Coffman's. And if Jim had come with me, I think the relationship would have been different. I mean, he maintained a different identity there, and he worked with Coffman, and so when he came to Oregon State, we had not worked closely together for a long time. And so, we got reenergized when we got back together. So that was pretty helpful, actually.

VR (14:55): What about Wayne Minshall?

KC: Oh, Wayne. Yeah, well, Minshall's probably the only real scientist of the bunch. I mean, he's an absolute scientist. This guy is incredible. And without Wayne, we would not have published the papers we have published. It's plain and simple. The main River Continuum paper, the 1980 paper was, I wouldn't say published over Robin's objections, but published with Robin's concerns that we weren't ready. And Wayne's insistence that we had to be ready, and the interbiome comparison papers and the big river part of that, this has all been driven by Minshall. He's a very insightful, very careful and talented

scientist. So, he was kind of the real scientist in the group, I think. And he's very different. He's a lot more methodic. He and Sedell are great counterpoints to each other. Sedell is everywhere, and Minshall is really good at "Okay, here's a task. We gotta do this. Let's bring all the important ingredients in, but by God, let's get it done." And we would not have survived without Wayne, there's no question. You know, Burt kept us talking to each other, and Wayne kept us publishing. And otherwise, we would have been dead in the water. So, it's a remarkable group of people, and we were really lucky to have kind of found each other.

VR (16:29): Do you think that the public, the scientific community was aware of the different roles?

KC: Not at all. Not at all. I'd say that Stu Fisher and Jack Webster, who were the outside reviewers for the project, they certainly knew what that was. When we would interact, it would be clear, but from outside, I don't think anybody really knew what was happening. And that's probably the best thing you could say about us. That that was seamless from the outside. And that means that we had worked this all out together, and then we presented this, as we built the story together kind of collectively, we presented it collectively. Even though we would disagree intensely on certain things working together, but outside, if something like Winterbourne or something came up, that we would really circle the wagons and operate in a very unified fashion. So, it was a family type thing, there's no question about it.

VR (17:39): The classic image that I know students have of the River Continuum is of the diagram. How did that diagram come up?

KC: Well, you know, that is a sort of embarrassing story in a way. We had talked about how would we, sort of, visable-ize or visualize this sort of thing, and so I drew this diagram, and I was invited to give our paper at a symposium on the Sanduskey River Basin, an obscure little grey literature thing. And so I drew this diagram up, and I cut and paste a bunch of things, you know, put it together, and presented it as a slide at this symposium. There were maybe fifty people there, all interested in the Sanduskey River. Then, they published those proceedings in their own little bulletin, a real super great literature thing, and that predated the paper. We had all agreed that we were going to fire this out in this paper, and that's it. Well, I thought most of these symposium proceedings, it takes years before anything ever happens. Well, they turned this thing out in like three months, and there it was out there. So, these guys started calling me Scoop Sanduskey and a bunch of things, and that was never the intent. I was just throwing this out for the audience to think about. So anyway, that thing came out, and because of lot of people had seen that, then we thought, OK, well we'll put this thing in our paper.

VR (19:18): Yeah, well it's almost the emblem of the River Continuum.

KC: It really wasn't intended to be that way at all. It just happened. And there's a lesson there. The things that survive your paper forever are the figures. If you get a good figure that supposedly is a model, that's what's in everybody's lecture, this kind of thing.

You know, we all look for that. "Oh man, is that a great model. I'm gonna get a slide of that so I can show my class."

VR: A picture's worth a thousand words.

KC: Yeah, no kidding! It really is. Well, I mean, look at Lindeman's drawing. My God. I mean, it's been there forever. It's not even right, but it's been there forever. But it's beautiful.

VR: And Angule's curve.

KC: Yeah. I mean, exactly! These are the things that, man, they just imprint. I guess we're all pretty visual.

VR (20:11): Is there any direction that you'd have liked to see the RCC go that it didn't go in, either because of lack of funding, or you all did other things?

KC: Um, you know, I wouldn't have pushed this at the time, but I think it would have served us well for us all to have spent a lot more of flood plain time, thinking about flood plains. The problem is that when we looked for a site to do the whole series from headwaters order one down as far as we could go to eight, as we ended up in the Salmon, all systems we looked at, the first criterion was that it must not be damned. But, the other criterion that we didn't pay a lot of attention to was that it must not have been isolated from its flood plain. Well, the Salmon, a lot of its isolated from its flood plain because it goes through a canyon. But we really didn't think that out carefully enough. It was there. We all talked about it, you know, and in fact, we attended a meeting at Friday Harbor on the Amazon, and that was all about the capture of flood plains by rivers. And I think it would have served us well to spend more time thinking about the lateral aspect of the system. But the ones we explored, in consideration for what one to use for the big project, these were all riveted banks. I mean, I can remember, we were talking about this, trying to get our general riparian picture together from the headwaters down, and I called the Army Corps of Engineers in Blacksburg or Vicksburg or wherever it is, Vicksburg, I guess, and said "I'd like to get your data," because I'd heard they had a lot of data on riparian corridors, and they passed me around from one colonel to another, and finally I got this guy who was the head engineer, and I said "We're looking for information on the riparian zone," and he said "Oh yeah. We do a lot of work in the rip rapped zone." And so that's what we saw everywhere we looked at. We saw rip rapped banks. We really didn't see any kind of flood plain, because they'd all taken care of that. They'd diked it and levied it and stuff. But yeah, that would have been...that's definitely an area where we should have thought a lot more about. And then, the general effect of tributaries and tributary junctions, which is now...a lot of people are now talking about as being an organizing principle. We thought about that, and in fact, we did a lot of measurements about that, but those are kind of two areas that we'd have been well advised to do more. The Continuum, when it ran out of funding, it just kind of stopped, you know? We were all still good friends, and we all talked to each other and had projects and so on, but there was no...it's the way we do science in this era. NSF established a long term ecological

monitoring program and what they meant was “We’re not going to fund in two-year increments, we’re going to fund in five-year increments.” Now still, they don’t get it either. To do this kind of thing, you need this long record, and there should have been some other...you know, we had a lot of students, they should have come in behind, and just kept going, because there were lots of things to do. So we kind of failed in that area, I think. But you know, the science is driven by the funding in a lot of ways, and we’d run that string out. Nobody wanted to talk to us about that.

VR (23:45): Of course you know, the flood plain with Wolfgang Jung followed that, and it’s something that really fit in well with the idea of this concept.

KC: Oh yeah, for sure. And I think the value of the River Continuum Concept is not so much whether it was right or wrong, it’s that it got people thinking about a lot of things, and doing a lot of things, and looking for exceptions. There were people who were really out there looking for exceptions, and we learned a tremendous amount from the exceptions that they found. I think that’s pretty telling, actually.

VR (24:18): One of the things that I just thought of is that if you think of all the spin-offs that just developed new names like the Serial Discontinuity Hypothesis, did that bother you at all, that people were putting a new name on a slight modification?

KC: No, no. Not at all. I don’t think, well, obviously I can’t speak for the rest of us, but the kind of ownership we felt, I think we felt ownership in the group and maybe some of us have felt backed into feeling ownership for the concept, but I don’t think that was really the issue. We were anxious, we tried hard to get other people to come in and think about some of these things. I think we all felt that that was pretty healthy. When Ward and Stanford started talking about “Well, what happens when you do interrupt that?” we said “Well this is...we should think about this. I mean, sure, let’s see what happens.” And sure, now it makes sense if you think about how to reset a system.

Mike Furniss (25:35): Speaking of resets...I just ran out of tape.

Vince Resh: Ken, have you been genuinely, or generally pleased about the way the RCC has been used by others for management?

Ken Cummins: Um, I guess I’ve been pleased, but I’ve been unhappy with the fact that it actually hasn’t been used as much for management. Recently, the Elsevier series on ecosystems of the world, biome 22 is on rivers, and Wayne and Burt and I edited that, and it’s being reprinted, and we’ve rewritten the introduction to it. In rewriting the introduction, it’s pretty clear looking at the literature that some people feel that the RCC has been an impediment to management, in the sense that it’s sort of put sideboards on the thinking process and not allowed...I’ve been disappointed about that, that people either feel constrained or feel that others have been constrained in the management arena by it. But I think it has had...it’s played a part in the general swing that’s gone to thinking about watersheds instead of...again, don’t compartmentalize, but have them...tell them to really start thinking about whole watersheds. There’s a whole host of

questions we simply can't address. So I've been pleased at the fact, whatever part the Continuum played in that role of getting us to a watershed perspective. We should have divided our country into water basins, not these stupid state lines. All this would have been a lot easier. There's sort of a yes and no about that. But I am unhappy that people think that it's been an impediment, because I felt the one thing that we did contribute was get people talking to each other and thinking about things, whether it's right or wrong. And there's not really any right or wrong about this kind of thing anyway. On the whole, I think it's been helpful, actually, and I take solace in that.

VR (2:21): When you were doing the River Continuum, was this the main research direction that you think you had over all those years, really going into over a decade?

KC: It certainly was at least the main context of what I did. My contribution to the Continuum, I think, was to talk about the functional role of the invertebrates in the system, and using the invertebrate functional group approach as a surrogate for ecosystem function, so most of my stuff that was going on was really focused on how do we bring invertebrates into the picture. We first flew out the functional group idea in '73, and I've been working on it ever since. At the time the Continuum was going on, I had a fairly significant zooplankton program going, with Leptadora, a big predacious zooplankton, and so I had a number of students who were working on zooplankton, and it was kind of diverting, in a way, although they were mostly working on the energetics of zooplankton, so that was not completely incompatible with the River Continuum...sort of energetic approach. There was definitely an invertebrate focus in my lab always, and everybody in my lab got involved one way or another in the Continuum, because it was the major driving force, no question about it.

VR (3:58): Just one last question. You've had a great career. If you look back over your whole career, what do you think your biggest contributions have been?

KC: I think there's really two that stand out in my mind. One is the functional group approach. I remember that I talked to Victor Shelford when he was still alive, and he said "The species is the basic unit of ecology. You can't address questions until you've got that." And I said "Well, then stream ecology's in big doodoo, because it's going to be a long time for invertebrates before we can ask questions at the species level." We can't ask big scale questions anyway, because it's going to be really narrowly focused, so I thought "Well, what's the alternative?" And the alternative was that instead of every system we ever used asking "What is it?" we had to start asking "What does it do?" So, the idea was "Aren't there some ways we can look at invertebrate communities, and based on what they're actually doing, and how they're related to the habitat" and so on. I think that taking the systematics as fast as you can and as far as you can, but always having at least something. With the functional groups, saying when you leave the field, you've got some data, you can put it in a bottle, and later on you can do the rest of it, but the idea's that you can get the big picture and you can do it relatively quickly, and that there is some insight there, if you let the bugs give you the insight. So that, for sure. And I think the other part, for sure, is the part that I had in the Continuum. That is the major thing. I think that's the kind of thing we're all going to leave behind. So those two are

not unrelated, the really went hand in hand, and were kind of nested, but that, really, I would say.

VR (6:04): Could I just, maybe, ask a little more in terms of your personal life?

KC: Sure.

VR: What gave you the most satisfaction, of all the things you worked on?

KC: Um, in a general way, it was whenever we had a team together. When I was a Michigan State, we had a group of five or six people that worked really closely together. Much more focused than the Continuum, that was really enjoyable. We went out in the field together, we worked together, everybody helped everybody else with their own little projects. When Bob Peterson wanted to do a field survey of *Nigronia*, we were all there, all day long in the snow and freezing and so on. No matter who was doing what. And so, that was the best team experience that I had up to that point. And then I thought after that, "That's the way to really do it." And that's what attracted me to Oregon State. When I went from Michigan State for a sabbatical, they had really played that way beyond what I had done, because now the teams, who were all almost family, had such different disciplines. I mean, you can't be in the field with Fred Swanson for longer than ten minutes to know that not only has he figured a lot of things out, but his whole perspective was really different. That kind of group dynamic was already there. And the stream team was the forum for bringing all these people together. Once a week, every Monday morning at nine o'clock for what, fifteen, twenty years? Every Monday morning these people get together and talk about stuff? That's remarkable. That's really remarkable. And I thought, "You know, this is the way science really...that's fun. That's really fun." And when you're in the field, you have a good time. We had great times in the field. I mean, it was a lot of fun. If this isn't fun, then you shouldn't be doing it. I worked in Europe long enough to know, really, what sober science is like. I mean, these guys, they don't laugh at anything. How do they get through it? I mean, you can't slog in the mud all day without laughing a bit, or else you're going to go nuts. That sort of family, science family was really the stuff that gave me the real kick, and it still does. That's really fun.

VR (8:30): Really, when you went to Florida, that was the intention. That you were to try something similar.

KC: Oh yeah. And initially, it was. When Nick Aumen and the others, we all got together, hell, they had thirty biologists in that group, and that was great. And geologists and hydrologists, I thought that was where we were going. It should have been where we went, but it just kinda came off the rails, I think. Too bad. But that's really exciting. To tell students that this is the way it should work, and in grad school, the graduate students should be experiencing this, but boy, you know, I see them getting focused on their own project, and you don't bring them together in a real kind of group. There's a group dynamic that's so exciting. If you're out there, and you're all pulling on something, I mean, that's really fun. I got a big kick out of that stuff. It was great.

VR (9:33): One specific question. Did you coin the term “functional feeding groups?”

KC: The way that worked was the closest analog is guilds, so I did a lot of reading in the guild literature, and that just didn't fit. The guilds aren't quite the same. And I don't frankly remember how that came about. This is the kind of thing that conversations with these guys probably brought that out, and I wouldn't be surprised if it wasn't some term Robin slipped out. Because he would just drop these gems, but I had the idea that we really need to do sort of a guild approach, like the terrestrial ecologists and terrestrial entomologists, you know, all the animals that feed on the same plant type of approach. I thought “We need something like that.” Function was what I was after, so it seemed reasonable. Of course, that is what Hawkins has called “Fictional Groups,” so I left myself open to that one. It just sort of grew out of reading the guild literature.

VR: What a way to start. Perfect.

Robin Vannote Interview

VR: Robin, thank you very much for coming out. You get the award for the longest distance traveled. Tell us a little about your early life. I know your father was an entomologist. What made you become a river ecologist?

RV: I guess my interest in streams and rivers began at a very early age, like all kids who walk in puddles. And I always had streams nearby, so I always walked in streams. As a kid I spent a lot of time collecting things in streams and bringing them home and I think I was always fond of wandering around, many times alone, just walking through streams, just wading down and picking off the leeches, sort of thing. So I always felt comfortable in streams, but I guess my first real formal look at streams, intellectually, was in high school I came across a couple of books. One by ?Needham? from Cornell that wrote *Life of Inland Waters*, so I studied that book in high school and I really liked that. And then there was a trout restoration book by a fellow from New York State and I thought that was really neat. So I think in high school I was already focused on streams as something I was really interested in. Some of my high school summer jobs were working for an entomologist on the experiment station at Rutgers, measuring the emergence of ?mandesonian decaigence? in freshwater swamps. I was just wading around swamps. So I think probably these early exposures to water, whether it was ponds, swamps, springs, or beaver ?flowages?, they were always sort of interesting.

VR [3:59]: Did you study biology in college?

RV: Yeah. I went to the University of Maine in Wildlife Conservation and I quickly aligned with the Fisheries department and the Zoology department at that time. ?Harry Eberhard? was the Fish Manager professor ichthyologist and wrote the textbook at the time on Fisheries Management. So he sort of took an interest in me because I was interested in streams. He was a Cornell man and my dad was Cornell. So, he sort of looked after me. And I think that making a connection in school is so important. So I had summer jobs when I was in college working for the Atlantic Salmon Commission. I was a field biologist. I think we got \$40 a week or something like that. Trying to figure out how to bring salmon migration runs back to Maine streams and rivers was a real challenging job that this fisheries biologist had ?on the Sea-run Atlantic Salmon Commission. I thought, "Well, this is what I want to do. I just want to be a fisheries biologist and work with Atlantic Salmon." You get a couple of summers of work at that you can see the paper and pulp waste, the dams, it was just a challenge that wouldn't be a productive career. It was just, Maine was impacted by stream pollution. And then I got encouragement from Harry Eberhard to go to graduate school. He listed of a number of good ones. Fortunately, I picked working under ?Bob Boln? at Michigan State because he was doing some real interesting stream work at the time. It was either going to Cornell, Michigan, University of Michigan, Michigan State, or Oregon. I think the assistantship under Bob Boln...was the most attractive. I was supposed to be a TA at some other place. It was a marvelous choice. I think I spent quite a few years getting a PhD, but I think the time was well spent because I really took advantage of every course that was available that I was interested in: physiology, chemistry, soils, just a wide array.

I really think that is an enrichment. I think too many people are too anxious to get done and out and don't really take advantage of what's available at university courses. I think that added enrichment is really good. I think the working with Bob Boln, he was sort of a pioneer in looking at nutrient spiraling by his experiments on the ?pigeon ruber? I think P32 looking at it's updates in periphyton and recycling, so that was sort of the first spiraling study conducted. That had some really interesting humorous side stories...spilled in the woods.

VR [8:40]: Your professional career I know took you to ?TBA? and then directly to Stroud from there?

RV: Right, I spent three years as supervisor of the biology section and I think that exposure was of tremendous value, too. Seeing really how big river systems work, how ?impounded? rivers work. And I was in an engineering department, essentially, it was held in public safety, but it was all run by engineers. They taught me early on that you couldn't understand how a system worked unless you understood how the physics of the system. You had to understand how water flowed, how it separated, turbulence, you had to understand the physics of a system and its operations if you wanted to know something about the biology, circulation patterns and reservoirs, and released water. So it's very instructional.

VR [10:01]: How did you come to go to Stroud?

RV: I was doing a big study on the Green River in Kentucky with TBA, they had just put in a very large esteemed power plant. There was a gross error on calculating the amount of water flowing in the Green River because the locks'd been raised and so the engineers thought there was much more water flowing in the river than there was. Just shifted the curve relating ?steps of flow? and to make a ?congregational? river and so it was a vastly oversized plant for the Green River. I think when they first turned both units on the river went up to 112 degrees and never saw so many dead fish in your life. It was just a very interesting experience and then they brought down the academy teens to study the river.

VR [11:20]: ?Stan Roebach?...

RV: Yeah, the whole group came down and I met Ruth and whatnot, but I think Ruth had contacted Bob Boln among others who might've been interested in the Stroud job.

VR [11:46]: You were the founding director, weren't you?

RV: Yeah, and so I moved up there in the summer of 1966 I guess, not knowing what kind of budget I'd have or what was really going to transpire but I knew that we'd developed some kind of research laboratory. So I essentially spent the first six months designing the lab, trying to find a place to ?site? it. Looked at a number of different streams, found one that was most suitable and then the Stroud family bought the farm that the site was on and became contiguous with some of their existing property. So there was a number of different, I guess there was three different streams I was looking at.

And then we started designing what size building, what was going to be in it. I was very modest and didn't design a very large facility and Mrs. Stroud said, "Don't you need something bigger than that?" I was sort of designing it after a small research lab that the ?Haas? family had built for the biology department at the academy on one of their properties. So, I think Joan Stroud had a lot of inspiration towards doing something for a significant rather than just having a station with a few people. So, I think given the opportunity to find something very substantial and long-lasting. The idea was not to build a 40-year building, let's build a 100-year building. Let's make it aesthetically pleasing. So it really became a family project in a true sense.

VR [14:23]: When you started at Stroud or maybe before, when did you start getting the first ideas about the Continuum, I remember you said a little earlier about your studies on White Clay Creek.

RV: Well, I think when I first got there and ?Paul? was 66, that's why I first started putting together leaf packs and putting them in streams. I sort of got that idea when I was working with TBA and ?collecting? in smaller streams and I had sort of a side project then. I did on the way home from major field work and I got very intrigued with the amount of leaves in Kentucky streams. The insect community in those, but I'd never had an opportunity to explore this any further, being a government employee. So, that's the first thing I wanted to do. So I ran that by ?Edwin Hutchinson? what he thought about this as being a viable project and he thought it was very interesting and he encouraged me, so I started out sewing leaf packs together and trying to estimate how my leaves were flowing in streams. I put in some ?naive weers? ?electric? and found that storms were very ?perilble?. Even small ones. So I think looking at the Detritus leaf little essentially, that's all I was working with at the time, from headwaters down to the third order stream, sort of got me looking at more than just a station by station analysis of something.

VR [16:25]: The ?geomorphology? you brought into the Continuum, was that self-taught or did you have that back at Michigan State?

RV: Well, during my studies on the Red Clay Creek" in ?Red Cedar? River in Michigan, the last year of that work was the first time that bulldozers started tearing up the landscape for the interstate highway to Michigan. And the Red cedar had eleven tributaries coming in from the south and two from the north, so they put the highway on the south side and every one of those streams became a ?conduit? after every rain storm. It was just a terrific impact on sediment accumulation, and here I was trying to look at a small mouthed bass nesting. It was like a disaster. But I think I started measuring depths of sandbars in the streams there. But I think it's part of just understanding the physical geometry of channels. And I would guess it's ?Reds Lowman? and ?Luna Leopold's? discussions with...and they would come out. They were available. So, our guess with the TBA work, I was just ?engrained? to think that, understand the channel. Doing any study on ?channelization? for the ?Counsel of Environmental Equality? just exposed me to a lot of what tremendous amount of dynamics goes on in a channel after it's been gutted as it tries to weakly restructure itself with sediment, but it really can't because it's confined in it's narrow channels, so it's just always shifting. You can see the dynamics of seasonal

changes in water on the shifting of these sediments and there's just no stability because there's no vegetative shield and you can't have stability if you don't have that vegetative shield. The Pennsylvania Piedmont streams, the uplands are largely in agriculture and the hill slopes in the wetlands are in woods. And it's essentially a 20:80 percent ratio and it's been that way since the early 1700s, because William Penn and ?Steve Olovgenes? required that every farm had to keep one to five acres of woods. Because he recognized the problem they had in Europe. No, and that's the way it remained in Piedmont, Maryland, Pennsylvania, there's still that ratio. But the problem is the run-off from the uplands is always diverted to woods. And woods erode terrifically when water's diverted through them. So that was one of the challenges of studying in those Piedmont streams was how do you route water into streams when it's always going to run off agricultural fields in the wintertime?

VR [20:31]: This wasn't part of that ?CEQ? grant, was it?

RV: No. But I think these were all the things that you see and when you're out on these studies of What happens when vegetative shields are disturbed? and What happens when there's no vegetative control within a stream channel or even removed the large boulders and obstructions in the channel because you're only interested in maximizing the delivery of water.

VR [21:05]: Now then, you'd said that CEQ grant and I just want to ask you another question: That brought you all around the United States looking at streams. That was...

RV: It didn't go to every state, but it went to 42 streams that were distributed pretty widely. And the work actually involved studying the documents leading up to the work. Why was the work done? And then going out, meeting with the project sponsors, the ?SCS? people from the state and the county and perhaps someone from Washington, depending on the project, and then I went back and did a kind of a biology study of What was in the channel? and What was in a similar channel nearby that wasn't disturbed? And this was an eye-opener. This was important in knowing how streams are similar across a lot of different geographic areas. And we're all trained that channels are constructed to convey water and sediment. That's their purpose. That's why they're there. And then the biology has to fit into these mechanical and chemical parameters described in the channel. So that's why I had to really understand the physical dynamics of the system as well as the biology. I always encourage students to pay good attention to both.

VR [23:08]: What I wanted to ask you was, the different people that were on the Continuum with you in terms of what roles do you think they played? For example: Bert Cushing. What was the big contribution that you think he made to the Continuum?

RV: Well, he was always interested in primary production aspects and the creek project I guess that was, that and the insects sort of the major things he was working with. I think he felt very comfortable in taking over with ?Tom Bodd? in looking at the developing methods for measuring its primary production. I think Tom really took the lead in it

because he had been doing extensive work up and down White Clay Creek system. Tom was a very invaluable person in this group too.

VR [24:14]: What about Ken?

RV: Ken had all the energy and ideas and he's a worker. He's a valuable component, whereas Jim was the spirit, of course. And enthusiasm and "Get along with" and "Let's get moving". Jim got people working and Ken had a lot of knowledge to bring to the project.

VR [24:49]: How about Wayne?

RV: Wayne's a work dog. He's working like a student. They work work work for Wayne. I think Wayne's probably the major driver of the project. He has the self-discipline to really keep focused and demand stuff out of himself and he only gets distracted by his farm work.

VR [25:26]: In our talk this morning, you'd mentioned about that AAAS symposium. Would you say that's really where the group started? With that?

RV: Well, I think that's where the group got introduced to one another and found out what was going on, what other people were doing, what were some good ideas, what could I take home from this and try. I think, for me, it was meeting some people that I hadn't met before. And a chance to tell what I had been doing, what I've started. It was a good trip. Too bad they didn't put the right spelling on the front cover! Bob Boln I apologize for that. I forget what I misspelled, but I remember spelling.

VR [26:27]: Do you think that the rest of the scientific community was aware of what roles each of you played in the RCC? That Wayne was the work horse and Jim was the one that kept, or do you think you were viewed as one big group that just, everybody did everything?

RV: I don't know. I just didn't let it bother me. I had a lot of things going on, a lot of management things to do, a lot of personnel things to do, a lot of "cheat sheet" things to do.

VR [27:04]: When you were doing this, this was not the main thing you were doing, the River Continuum, was it?

RV: I just had a lot of things going. We were continuing our White Clay Creek studies.

VR [27:19]: And your lab was expanding tremendously, wasn't it?

RV: Yeah, after the Rockefeller study was completed...

VR [27:28]: The Rockefeller study was a...

RV: That was a three-year study for our ideas on what we want to do on White Clay Creek as a team project involving three or four scientists working on trying to understand the function and structure of a third order stream system. It didn't make it through ?IVP? review, but the Rockefeller Foundation liked it and they were thrilled with the results at the end and then they sponsored this workshop. And they also said they were so fond of our work, are proud of it, that they wanted us to be able to RR physical facilities ?slash?. So they called up the ?Crusty? Foundation and said they have a candidate and a couple of days out on a plane to the Crusty Foundation in Michigan and they gave us money to double the size of the lab and build a meeting house. The Crusty Foundation is just a brick and mortar. It's the foundations they rely on. Other research places like the Rockefeller Foundation tell them where to put the money.

VR [29:10]: Who in the Rockefeller Foundation were you dealing with? Was there an environmental group then?

RV: Yeah, his name was ?Richardson?. Ralph Richardson. I'm not sure other projects that he worked on.

VR [29:25]: Because in terms of environmental...

RV: Yeah, he was in a lot of agricultural work and he had a great fondness for Penn State University for some reason. Actually retired on a golf course around Penn State. But his son came to work for us and eventually got a PhD in marine biology. So that was interesting.

VR [29:53]: Now, Robin, were there any directions you would've like to have seen the RCC go into, any research areas, that either because funding stopped or people went on to other things that you didn't actually get to do?

RV: It's hard to kind of think back of what direction one would like to take it to. Because my time was really taken up by a lot of work with the studies of this geographical work on insect distributions and thermal equilibrium things which also fell out from the Rockefeller Foundation-sponsored studies.

VR [30:44]: Just what led to the third...

RV: Yeah, because we were doing so much work on metabolic rates on insects and growth rates and distribution within the drainage system. How temperature seemed to determine where species were. I would've been very interested in continuing the River Continuum Concept looking at stressed river systems to see how the efficiencies of systems might have changed with dropping out of species or groups of species and I think that would have a lot of potential in that area.

VR [31:33]: Especially when you think of all the biodiversity...biodiversity productivity.

RV: Yeah and structuring of populations and the evolution of the community structure I think are some interesting questions. And the base of the changes of water quality.

VR [31:55]: Have you been pleased with the way the River Continuum Concept has been used in management?

RV: I think you have to be pleased that an idea as continuum. An idea's the most cherished thing that someone comes up with. It's so valuable an idea. You really have to cherish ideas. And good ideas are more ?equitable?. You've got to look at every idea and the value and weigh it. You can't be too quick to dismiss ideas. They don't come easily. I would guess good ideas come more quickly to people that have a broader background in their education and more experiences. And then I think testing the ideas out with other people. Seeing whether we ?get funny looks? or help in fleshing out those ideas to something more rigorous and bigger question.

VR [33:32]: One of the things I think happens whenever something is successful, like the River Continuum Concept, is people take it and modify it slightly and put a new name on it. Serial Discontinuity is one that comes to mind. Do you think that was, did it bother you, was that okay to you that people were...

RV: Sure, I mean, who knows, maybe somebody will search back and find the River Continuum as it was described somewhere else with a different name. I don't know. People, I think, always want to find a different adjective or noun to add to something to make it fit their needs a little better. Ideas evolve, what's a good idea in one decade a decade later is built upon and made better. So, ideas like that become community property pretty fast. And should evolve to be more rigorous or be applied in a broader manner. So that's healthy and good.

VR [34:50]: One of the like, almost the logo, of the River Continuum Concept is the diagram with the stream with the functional feeding groups changing over time. Were you surprised that that's become such an identifiable, that graphic's become so identifiable with the concept?

RV: I think that even the design evolved, logo's evolved. I think looking at the ratio, the stream geometry, the ratio of lengths of widths and depths and these sorts of things are something that just took terrific numbers of measurements, with and without vegetative control, with or without a canopy. How these shift so rapidly where you can have a first order stream that's narrow in the woods and you put it out or wide in the woods and you put it out in the meadow and it's narrow and how grass ?encroaches? on streams. Just the dynamic changes I think just naturally lead you to just a simple design and you can hang the Christmas tree ornaments wherever you want, but it's just the idea: this is the ratio for streams to increase as they gain more water and energy.

VR [36:20] It's just that shape...

RV: Yeah, it's not necessary to throw a tributary here or an insect here or a fish here. It's really how water shapes a channel and then the biology gets in to the dynamics of the system and energy input.

VR [36:49]: Do you remember where the term "functional feeding groups" first came from?

RV: I think Ken must've come up with "functional groups". I think we had a discussion of how, when we were trying to figure out what we were going to sample on the ?bampost? the invertebrate communities. The ?standard? sampling size is either a ?hess? or a square foot sampler. And then randomizing these samples in ripples or pools or some technique. We had to come up with some ideas. I guess on the, I had been using the, more or less, the square foot or the hess sampler on a lot of the earlier studies. But when I got into studying the insect communities that were temporally and spatially sequenced, I found it more useful to use some terminology I came up with: Distinct Topographical Units. So I had communities that grazed algae off the top of rocks. So that was my pet thing or they worked in lead packs or they were on sand or silt communities. So I was looking at distinct topographical units. And I guess that's where I was visualizing streams. I think Ken was coming from the position, I think his was more of putting things into functional groups. I think that's the way we went as the final sorting of animals into what they were doing. What their employment card was. So, yeah, I think there's a lot of different ways you can express it and I think that was a good one for doing cross-biome comparisons. Yeah, I didn't know how I would handle distinct topographical units...

VR [39:27]: You've had a remarkable career. When you look back at the contributions you've made, what do you find the most memorable? What, if you had to single some things out?

RV: I think a lot of the enjoyment that I've had was really interacting with students. I taught a stream ecology class at the University of Delaware and really thoroughly enjoyed that program and put a lot of masters degree students through the department there. Most of my way is loving streams and on occasion I've bumped into a veterinarian that had taken the course Econ & Entomology and, "I'll never take a stream other than the way you taught it". So I think working with that. And then several students with Reds Lowman and John Hopkins and like ?Ellen Silverbuild? just gives me so much pleasure to see how her career went in toxicology. She worked on the interaction of temperature and ?dealdron untessilated darter? on White Clay Creek. I think the way her career just became spectacular it's a huge pleasure. I take a few other PhD students, like ?Bernie? he's really turned out to be a good scientist. So I think you have to have a lot of pleasure in that other people are working. We had some biologists on the staff that are outstanding people, like ?Dave Funkus?, an outstanding entomologist. He was a student at the University of Delaware. And he worked on the River Continuum Project and spent a year or more in Idaho. So I think studying the progress students make has to be a reward.

VR [41:54]: Thank you very much. This was a great interview, thank you. Thank you very much Robin, this is a really great interview.

Wayne Minshall Interview

VR: Wayne, thank you for being here. I'd like to start talking to you a little about your early life and especially what influenced you to become a river ecologist?

WM: Actually, that didn't start until after I got to college. I got interested in fisheries program, a fish and wildlife program, and the person teaching it, or involved with it, was C. J. D. Brown who was a Ball and Hooper kind of a colleague and his wife had been with P. S. Welch as his secretary. In the course of going through that program, I began to think ahead about graduate school and so on and I came across a paper that one of Brownie's students had written. Gumtow had done this study on periphyton in the Gallatin River and that really captivated me. During the limnology class, he took us out to the Gallatin, we did some sampling, I did a few projects relating to streams and I think I was caught after that. Brown, through his wife, (his wife, as I said, had been a secretary for P. S. Welch; so did one of the people at the University of Louisville and they had shared the office together), so when I expressed an interest in going on to graduate school, Brown put me in contact with these people at the University of Louisville, where they were starting up this Potamological Institute for the study of rivers. I said, "That's really what I want to do". I won a National Defense title IV fellowship to go to Louisville and began a pretty concentrated career then and studied on various aspects of stream ecology. It all took off from there because I did my dissertation work on Morgan's Creek. I got involved with Minckley sort of on a tangential thing where he and Louis Krumholz were studying Doe Run. My wife was working with Louis Krumholz on a Master's degree and so forth. So I just became more and more immersed in stream ecology at that point.

VR [2:24]: Tell us a little about, in your professional career you've worked in many parts of the United States and in the world. How did that influence how you viewed streams?

WM: Well, it was a very broadening experience. First of all, I came from the West, more or less arid but forested, where I was drawn to eastern deciduous forests. I had never been across the Missouri River or the Mississippi River before, in terms of going east. So this was a whole expansion, just in my education, to be exposed to that. And then the whole sort of paradigm that you build working with deciduous forest streams, and (contemporary with some of the other eventual PIs) I was coming into the realization of the importance of the leaves that fell in the streams in terms of an energy base. I ended up doing my dissertation research on the importance (the role) of allochthonous detritus in stream systems. So that was very informative in my view of, sort of my world view, of the ecosystem. Well, then I went to England where a lot of the trees, where it would have been deciduous forest trees, but the trees had been removed, clear back to the time that wooden ships were being built. That system was kind of kept in a disclimax by the sheep. So here we are in moorlands, going through some degree of influence, but where (influence by the deciduous vegetation) but where the grasses play a much bigger role, where the water chemistry was more modified by aerial inputs, and so on. So we had sort of an acid, low ionic balance and that ultimately turned out to affect a lot the distribution of the invertebrates in the River Duddon system. So, a little bit more mind-expanding

kind of a system. And then I came West to study in Idaho and I was drawn by the Rocky Mountains,...by that whole western revisitation of where I'd grown up, and so on. I was really intent on doing a mountain stream study and even... I began some studies on Mink Creek, which was near where Idaho State University is. Some of those continued during my career. We were approached by some people from Utah State who were starting to become involved in the International Program In The Desert Biome. I wasn't particularly interested and sort of unenthusiastically participated. But then, the person, Fred Wagner, reminded me that there might be funding available for research in desert streams and so I sort of grudgingly decided that perhaps I should devote some attention to that, in deference to studying mountain streams. As I've commented to others subsequently, I learned more in 5 to 10 years of study on a desert stream than I would by studying another 25 years on a mountain stream, just because of, again, the eye-opening nature of the activities and my realization that in many streams autotrophy rather than heterotrophy (or allochthonous input) was important. So that really gave me a very broad kind of world view when it came to participating in the discussions for the River Continuum. And a lot of those folks were coming from forested environments. I had had the experience in the forested environment, but now I had the perspective of the more-open systems and the potential role that autotrophy could have, even in the headwaters or, as we finally emerged in one of our publications, a "sliding scale" in relation to the river continuum.

VR [6:06]: Mentioning the River Continuum, how did you get involved in that in the first place?

WM: This always stops me for a minute, just to think exactly when (and maybe there were several kinds of incidents as it finally brought it about). I know that Ken in particular sees as a focus the AAAS meetings, and that surely brought us together; made me maybe friends with or acquaintances with Robin, with Ken, and with Bert. Bert and I actually struck up quite a friendship as a result of those meetings, even stronger than with the other potential PIs. But then, when we came back, I was able to involve Bert in the I.B.P. program, that was still ongoing, as a part of the extension of the cold desert, Desert Biome, sagebrush steppe sort of area. And then, through Bert and some exchanges we were doing among Oregon State, and involving Bert's lab, and involving Idaho State, and some traveling with graduate students to have interactive kinds of mediums, and so on. I gave a presentation on "autotrophy and stream dynamics" in our desert biome system, Deep Creek. And Jim Sedell was there, and I think that was the first time I met Jim. I really believe it was at that point that I, through Jim's efforts, got involved in the further River Continuum contacts. Because I had not been in loop for the Rockefeller meetings and the involvement that Robin had out of the Stroud Lab and I really didn't know the significance of the research going on at the Stroud Lab at that point. Even Ken's work at Michigan was a little more remote from Idaho State. So those two things, the AAAS contacts and Sedell: in this roundabout way through I.B.P. and eventually being asked to join the group in the deliberations, that eventually led to a proposal which we called the River Continuum Concept.

VR [8:16]: When you were doing the River Continuum research, was this the main focus of the activities of your lab room or were you doing other things as well?

WM: I think we all were probably doing other things. That's essential to keeping alive. And some you're carrying to completion as you're starting up others. And a few things you're trying to keep together as a hedge on the future as well. But the River Continuum became the focal effort and the other things were either put around that or constrained in some way by the efforts of the River Continuum. The I.B.P program was winding down, the funding there was drying up. So there was some completion of work there ... and some that sort of was left hanging. I had some work that had moved forward on the Mink Creek research which I hurried up and finished some of that to try to get it off to publication as quickly as I could. And then some other things that were kind of just put on hold as we began to become more fully immersed in the River Continuum research. Then after that first year or so of transition, it's almost like total immersion.

VR [9:31]: What I'd like to do is ask you about each of the participants in the Continuum in terms of what role they played and you think they brought to the project. And, of course, let me start by asking you about Robin Vannote.

WM: Right, and for each of these I won't do them justice because there's just so much to sort of fill in and the memory fades a bit, too. But each will have a high point, I suppose, in one's memory as to what they did. Robin was foremost among the idea people. He obviously originated the River Continuum Concept that we built the framework on and fleshed out. He had a strong geomorphological background and I learned my geomorphology from Robin that had a very important impact on my research and teaching into the future throughout the rest of my career as a result of that. Some real in-depth thinking and a far-reaching sort of view that was matched probably only by Jim Sedell. Jim came from a whole different angle. So for Robin I would attribute the real genius of generating ideas that were global in their importance. And by global I mean, as opposed to local, Far-reaching and over-arching.

VR [10:56]: How about Bert Cushing?

WM: Bert, as I've said several times during the interviews and so on, has just been a good long-lasting friend. Partly because we were close, partly because we sort of complemented each other's strengths. But Bert was sort of the steady base and he brought to us, I mean, he would just always volunteer and deliver whatever he was asked for, so he was just kind of, almost like thinking of an accountant, basically. Keeping us on task, bringing things together, and overall providing continuity and a friendliness among the PIs that was very very important. So in some aspects, he doesn't stand out as prominently as the others, but often it's that steady force that carries everything through the tough times, through the highs and lows, and that's what Bert really did provide.

VR [11:53]: How about Ken Cummings?

WM: Ken was another idea person and Ken's enthusiasm, his ideas, his "spark." That's really all [the group]... we've got several now, that we'll talk about, that this "sparkling" is a really important thing. Getting you pumped up and sort of moving forward; somewhat

like cheerleaders and very important in that respect. Obviously, Ken brought the perspective of the functional feeding groups and he and I had been in parallel looking at detritus and its impacts. He had been able to move far ahead in terms of the experimental side of it and with the experimental streams they had and the research team he built up there. So he was making a number of advances that were really important in our thinking and in terms of research design and the like.

VR [12:48]: And, of course, Jim Sedell.

WM: Right. Well, Jim has taken a number of shots, but he is the spark that got things going, he's the one I think brought people together. He's sort of like a firefly in the spark he delivers. He's moving around, flitting around, but also having a very good big view, big picture view, of what was happening. He and Robin, I think, complemented each other in that regard. Robin had a much stronger geomorphological, entomological background, but Jim had the energy and the view of a need to sort of look much more broadly than I [had] and [than] I think many of us were accustomed to at the time. He brought the strength of an acquaintance, a strong acquaintance, with Jerry Franklin and of a really productive and innovative team operating at Oregon State University. Each of these, Robin's group, Ken's group, and Jim's group, each had a real depth in terms of the number of other people working with them; whether they were graduate students, whether they were technicians, whether they were other scientists. All that came to bear that when we began to move forward with the proposal and the research.

VR [14:04]: Wayne, what do you think your role was? This is a tough one to ask answer right. What do you think you provided?

WM: I'm not sure because I don't think about that a lot and I don't dwell on that. I know that I was a steady force. First of all, the projects were run through Idaho State, so I became sort of the lead PI in some ways in terms of keeping the paperwork together and some of the nitty gritty. But I also, maybe incurred (I felt a responsibility) to see that some of this stuff was carried to publication. Sometimes I was left with the burden of carrying some of that to publication; at times when distractions occupied some of the other PIs. So, I think I did allow, I was able to allow us to deliver on the product that most scientists would look at as proof of whether we used the money wisely, whether our ideas were worth evaluating or not. Other than that, I think I was able to maintain a good friendship and a rapport among all of the PIs at a time when sometimes there were conflicts between individuals. So, I would often be able to talk to one and then go talk to another and do a transition between the two in either an information transfer or even smoothing over ruffled feathers so that we could come together and move forward together. So, in a different sense than Bert, I think I was a steady force in, what I felt, keeping open the channels of communication and effectively allowing everyone to move on with what we were about in terms of doing the science.

VR [16:00]: Do you think that people outside of the group were aware of what roles you were playing, or do you think they viewed you as just sort of a homogenous group of everybody generating ideas and everybody doing all these roles?

WM: No, well, first of all, I think people by their personality sometimes become dominant or become lightning rods. So some people will be recognized among a group, rightly so or not, but they're just sort of, "Oh yeah, that person obviously must've had a lot of influence over the River Continuum." I think probably they did attribute some greater roles by some investigators rather than others. Again, whether that's correct or not, and I would say not as correct as they might think. But I don't think they saw us as "The Force" or "There's the Continuum group" or probably be able to pick us out of a crowd "There's this wedge of people that are obviously the Continuum group". And we didn't tend to just hang together exclusive of other people when we went to meetings and so on. But I'd be surprised if someone were to tell me, or you were to tell me, "Oh yeah, it's the four or five of you that everyone says when there's a continuum thing 'That's who I think of' ". And I wouldn't be the one that I would name as being the one that they would focus on; the one or two. I think it would be a couple people that we've already talked about.

VR [17:30]: Are there any directions that you would've liked to see the RCC go in that, for funding or other reasons, you just didn't get into it?

WM: Yeah, for me, again, I have a much better ability now to do global views and to see a broader picture and to look into the future as to where research ought to go. But at that time, I probably still was more intent on making it better science. So I would've been interested in: more replication, in a fuller representation of the annual sequencing of this whole; in filling in what the macroinvertebrate production and biomass responses were in relation to the continuum, as the effort where I would've liked to have seen it gone, at that point in time. In retrospect, we really would've had a great opportunity to look at much stronger linkages with the riparian. We know that's important, we knew it was important all along, but we had to sort of stay with the wetted perimeter, as our focus. We know the importance in the Salmon River that's very true of the hyporheic, and of upwellings and downwellings, and I think, in retrospect, I would've liked to fill that in. I would've liked to have tested the larger river ideas in other systems, other than the Salmon River and looked at things in that regard. And I think we could've done a much better thing by.... (because of financial and labor constraints, we were studying transects).... I think we could've done upstream downstream sorts of reach budgets much as Stewart Fisher pioneered on the Fort River and things of that sort. So, as you see, most of the things that I'm talking about are just sort of doing that little piece of research better rather than taking it in all new directions. And that's a good place to say....., that's where I would've been at that point. I certainly have gone different directions since then and I've found some interesting new areas to make contributions in.

VR [19:52]: There have been several spin-off of the River Continuum that, for example, the Serial Discontinuity Concept, that basically change the Continuum on a ?reset? and then new names were placed on a lot of the ideas. How did you feel about that?

WM: I felt both gratified that they were helpful in one way; gratified in that they saw the River Continuum as a useful model and that they could tailor it to adjust to certain

situations such as a dams and rivers system. And I think, overall, those have been very profitable contributions, so even my negative effects are somewhat dampened by that it has proved out that those have been useful conceptual images. In some ways, it's sort of like saying, "Gee, I thought we already said that." Obviously, we haven't filled it out as well, but the essence of these disconnects and so on, were in our thinking. It's, again, we were trying to set up, more or less, a null model (the pristine environment) against which, when you got a deviation, you could say, "Yeah this is a deviation, because of the disconnect, the discontinuity, of this set up by the dam or change in retention that might exist." So, in a way, those are simply embellishments and refinements of an overall model. I think the same thing could be said against the Flood Plain kinds of ideas of the rolled flood plains and the ladder effect. That certainly has been an embellishment, developed as a very profitable way of looking at larger systems. But it's not something we hadn't considered. It's not only implicit, it's explicit in the original River Continuum paper, but it also was in an area that we couldn't test and we had no familiarity beyond a cursory familiarity with something like the Amazon. And one where again, it just seems to me to just dot some i's and cross some t's in the overall continuum rather than change that. But it's part of marketing, part of selling an article or research grant, to put new names on these. So whether you call it Serial Discontinuity or Hyporheic or RiparianCorridor kinds of concept or whatever, those are really simply sort of corollary to the River Continuum in my mind. Again, without wanting to lay claim to all of it, I think all of those have been very valuable expansions on those original ideas. Just as the spiraling concept was a very valuable contribution.

VR [22:57]: Have you been pleased the way the ideas in the River Continuum have been used in management? Do you think they've taken them as far as they could?

WM: In some contexts, you wonder if they've taken them at all, in terms of applying what we've found. There are several things that I can think of. I think the initial work and then the later work that the Oregon group did on the role of woody debris is a very important change in our way of looking at streams and the importance of habitat structure in streams. I think a lot of people now realize what they do in the headwaters will have impacts a long way down, but whether they then manage middle size rivers or larger size rivers with that in their minds, often makes me wonder if they're taking to heart what we've taught. And there's a whole lot of arguing now between "Headwater streams are underappreciated" and "Headwater streams are where the action is" and so just count them up and headwater streams are where the action is, but underappreciated? The problem at our time was we all came from headwater kind of studies and nobody had ever gotten beyond that. Nobody had extended made implications down the river system and so on. So I think maybe that realization is more appreciated now among managers and among stream ecologists, but we seem to have almost gone full circle, we're reverting now to "Headwaters, gee, we've got to put all emphasis there." I think a better balance would be appropriate. Beyond that, it's hard for me to judge just how widely. Mainly in part because that's not the literature and not the direction that my interests, which have been basic ecology, has really gone. Other than that, maybe one other issue is just "processing, that goes on in streams. And how much loading from sewage and from

nutrients and so on a system can take and how you can tweak it. I think there's a lot to be learned from the River Continuum in terms of those interpretations.

VR [25:23]: Wayne, you've had a great forty year career and there are few scientists that can match what you've done. What do you think your biggest contributions, that you're leaving for the next generation of scientists, have been?

WM: My biggest contribution and my biggest joy, I think, has been the work with graduate students. To me, that's one of the most fulfilling experiences in my life and the greatest opportunity: to work with such a variety of people, to see people's eyes light up, to see them sort of come alive, and take hold in the growth that they undergo in the process. That's really been a "mainstay" in my career and I take great pride in it, I take pride in the fact that I've had over fifty successful graduates and very few ultimate failures in terms of their role. So that's my legacy to the future, I think, it's what's in my graduate students and so on, and where I count most of my productivity. In spite of over one hundred publications and so on, that's kind of just, that's the result of the effort put out and that, in most cases, we put out. If you'll look at my publication list, it almost, there are very few single-author publications in my list. It's because of the interactive role that I appreciated and cherished, both with other investigators and with my other students.

VR: Thank you very much.

Bert Cushing Interview

Vince Resh: What made you want to go into aquatic ecology or stream ecology?

Bert Cushing: Well, on one hand, it's easy. On the other hand, it's strange, because my dad was an educator. He was in the University of Denver when I was making that big decision, "What am I going to do in college?" And he kept pushing me, "What're you going to do? Maybe we could go do some aptitude testing out here to see what you can do." Because quite frankly, I was a little torn as to what I wanted to do. The outdoors always had an attraction and most of that, of course, was through fly fishing. My dad started me at it about ten years of age. But then a great passion I always had was athletics. As little as I am, basketball was my favorite. And I thought hard of going into coaching, but I finally decided that if you didn't play the game, you weren't going to make it, probably, and I knew I couldn't do that in college. My love of fishing and so forth, well, fishery biology and that sort of field sounded like a good way to go. So I headed up at Colorado State where they had fishery and wildlife school. And even through my freshman year, I admit, I still harbored doubts. At that time, between your freshman and sophomore year, you had to go to a twelve week forestry camp, where you spent time on map-making, dendrology, silviculture, all these things, and two weeks of wildlife management, of which only one week was fisheries. So here's one out of twelve weeks in your field of interest. Before this camp, I said to myself, "I'll go to forestry camp. If I don't like it, I'm going to come back and change to phys ed." I didn't like it and I still didn't change. I mean, let's face it. I went out for twelve weeks, learning how to identify trees and timber cutting just didn't have it. But I stayed in it and I'm glad I did. So I'd always looked for a career in fisheries biology. Colorado Game and Fish Department seemed about then to be my main goal. Of course, the Korean War broke out just before I graduated and I ended up going into the service, always with the idea of coming back to do a Master's. Before that happened, and during my undergraduate, we also had to take a course in limnology. This really hit me. Not only the subject matter of more than just the fish, but the whole aquatic environment. It really turned me on. That plus the instructor, who was Doctor John Olive, now deceased. He was such a great teacher and the course was so that I knew that's where I was going to come back and do my masters. And I did. So, after the service I came back and I took my Master's in limnology. Even at that time, finding a position with research was pretty tough. But I did get an offer from the Montana Game and Fish Department to become a research fisher and biologist. This sounded pretty good. At that time, Montana was separated into seven districts with a Fish Manager in each district, charged with doing the normal fishery biology things: managing people and fish, trying to keep everyone happy. I was the first outside biologist Montana hired to become a two-man team in a district. I went to Kaispell and my title was Research Fishery Biologist. Well, that was a real misnomering. It could have been Highschool Technician for all the research we did. I spent half my life, I swear, mending gill net holes. It took about two and a half years for me to realize this wasn't what I wanted at all. So I started looking around for anything in research I could find. Said PhD. You had to have the meal ticket. Many years before, the first technical meeting I went to, the 1955 AIBS at Michigan State, I remember. Doctor Oliv introduced me to Doctor Donald Rawson was the leading limnologist in Canada.

VR [4: 54]: The Rawson Academy.

BC: Yeah. It's been named after him. At that time, in this little dorm room at Michigan State, Dr. Rawson said, "You know, if you're ever thinking about coming back to school, let me know." This is strange from a first meeting from an icon type. But, when I decided to go back to school and I got out of Montana, it really turned out two things: I was too late for most of the scholarships that were available. I decided too late in the year. But after writing a lot of letters, I was actually accepted at Michigan State. I don't know how many of you know this. I was accepted at Michigan State. I had the ticket to admit me to Registration and everything and I never talked to one person live. That really kind of got me bomb all I knew, and that's where I sent my first letter. But from then on it was just rubber stamp, "Here's the form", "You're in." And that kind of turned me off. So I wrote to Dr. Rawson and he offered me an assistantship right off the bat, so that's how I ended up at Saskatoon. During the time in Montana, when I really got interested in streams, merely by being in a lot of them. Not a chance to do research, but I knew I wanted to do my PhD in something to do with streams. So, that's where I ended up. Now, that's sort of from start to where I ended up getting my PhD.

VR [6:36]: What did the influence have of Colorado State, of Montana, and of Saskatoon, and then, of course, when you went to Battellel. Being in these different environments, how did it effect how you viewed streams?

BC: Well, quite honestly, I didn't get a lot of exposure to what you would call stream ecology until I got up to Saskatoon. Colorado State, my Master's had to do with a toxicity study in lakes. I didn't work in streams then. As I say, I really got turned on to streams just on my own, not in any formal education or formal training in that direction. Every place you go you get a different perception of things. I like to tell a story about in Colorado, in ornithology. We were introduced to this bird out in the meadows and it had a yellow head. A yellow headed blackbird. I got to Montana and I learned they called it a black-assed yellowbird. So I like to use that as a way. You get a different perspective in different places. So, really, I first started getting really interested in streams when I went up to Canada.

VR [7:52]: How did you go to Battellel?

BC: Battellel. It's an interesting story. When I came back from the service to get my Master's. Let me go back a little further. When I was getting my undergraduate's Fisheries degree, there was even a degree in Fish Management when we started. In our sophomore year, they introduced a Fisheries major. And there were four of us, then, that continued on and we were the first four Fisheries graduates from Colorado State. Two of them never followed the field. The second one, Dale Hoffman, and I came back from the service to get our Master's. At that time, we didn't have a subject and Dr. Olive found us a real neat project we could work on together. So, Dale and I were the only two that followed in the field. While I was in Montana, after I got my Bachelor's, I was encouraged by an acarologistt into my PhD. Colorado State...

VR [9:02]: In entomology?

BC: Acarology. Yeah. Tyler Woolley. Best teacher I ever had in any school. Anyway, I said, "No, I want to get my feet wet in the world. I'm not ready to go right back and get a PhD. I want it to be in something I know I'm going to be interested in." Dale, in turn, stayed to CSU to get his doctorate. While I was up at Montana in Fisheries, for a summer job, Dale went up and worked in the Hanford biology labs. Hanford, Battelle, all this is the Hanford Nuclear Reservation. GE ran everything when I first went there and then Battelle finally took the labs. So, to get that straight. Anyway, Dale went up there, and we, of course, corresponded a lot and he said, "This is a pretty fascinating place. You know, they're doing all this stuff with radioisotopes and artificial streams and it's just really cool. This guy named Jerry Davis is my boss. He kind of got me pretty interested in this." And it just happened that while I was at Kalispell, it turned out that Jerry Davis came over to give a seminar at the Flathead station, which is just down the shore from Kalispell. So I hopped in my car and went down to hear this and it was just fascinating that they traced the radionuclides in the Columbia River to... And it just seemed like, "Man, there's a field of research that's going to open a lot of things. Just because you can make such tiny amounts of all these things." So I'd always kept this in mind and when it was time to get a job and I was finishing my PhD, I wrote to Jerry. I also wrote to Stan Auerbach at Oakridge. That was a second site that a lot of radioecology work was being done. Jerry offered me a job there, which I took. Interesting this was, about six months later I got a letter from Stan saying, "Are you still interested in the job down here?" I thought that was dead. Somehow we lost correspondence. So I went to work for General Electric in the biology labs, which became Battelle. That's how I ended up down there with...

VR [11:34]: You were there for thirty years?

BC: '61 to '96, thirty-five years.

VR [11:41]: Thirty-five years. Wow. How did you get involved in the River Continuum project?

BC: That's Sedell. I'll say it directly because he made the first call. As we reviewed yesterday and talked about it, I'd met Wayne and I'd met Ken by then. We'd corresponded in common things and streams, but it was a phone call from Sedell saying, "Can you come to Gull Lake. A bunch of us want to get together and develop a stream focus group endeavor. Would you come?" That was how I got involved.

VR [12:23]: When you were doing the River Continuum work, was that the main focus of your research or did you have a lot of other projects and duties you had to do at Battelle?

BC: Actually, on a percentage basis, the Continuum was probably less than ten or fifteen percent of my support. Realize here (and I'd like to be on tape) is that, as we said yesterday, we started out looking for something that the five of us could do at our sites

and our streams. What eventually shook out didn't include Hanford. The Columbia couldn't be a continuum. The first order desert streams that went back to desert wouldn't work. The Yakima, essentially duplicated, Wayne's. So, right then, they could've dumped me. Really. But they wanted to stay together as a team and I appreciated that. It was an honor to work with that bunch of guys. I'll never have the stature as them (and I admit it) but it was great to be with them. As a result, my percentage of the pie was very small. In other words, here's four sites, here's the pie we've got to cut. So they cut it in about four big pieces and a little sliver. That's where I fit in. No, I had all kinds of other works that was due on the Columbia River and Rattlesnake Springs and other sites. And of course, the real biggie by then, were environmental impact statements. In fact, earlier, when I first started there, 100% of my funding was from the Department of Energy, the old AEC. And they funded all of our radioecology in the Columbia, Rattlesnake, whatever we did. Working on the pure aspects of stream was very foreign. I had to pirate a lot of that and then when the EIS business came along, they cut all of our research funds they said, "All you guys are going to do is write environmental impact statements for nuclear plants." So for several years, that was a good part of our business. I was an iconoclast, I didn't fit into Battelle. They wanted you to do contract research that sold something. And, to the day I left there, I didn't get very much support from Battelle, because bull-headedly I said, "I want to do pure research. I'm going to do it some way." It cost me. It cost me in the organization, but I did what I wanted to do.

VR [15:07]: Let me ask, in your opinion, what each of the members of the Continuum group contributed. What about Robin? What do you think his main idea was?

BC: The main idea. The concept. The base. And that practically says it all, because if we had never gotten that story line out, as I indicated yesterday. We did a lot of wheel spinning up until then. And it was almost, to me, like someone come up with something like this, it was almost like Robin got tired of hearing this and he says, "Look here." And he got up there at Stroud and started expounding and everybody's eyes open. Sedell says, "Here's the story." So Robin, that's the biggest thing. Everything else, he contributed a lot, but boy, that was it. That was the main thing.

VR [16:07]: How about Ken Cummins?

BC: Ken was just smart. He just knew his ecology. I can remember Ken would get frustrated at times with detail, and there was a lot of things that happened that...how do I say this diplomatically...one guy'd be leaning this way and another guy wouldn't quite like it that way. This happened in all of us, Ken too. But he contributed so much intellectually to it, that quite honestly, Wayne and Ken, to me, were the real intellectual stimulants. They were the guys that provided a lot of the detail. Jim was the big idea man. I mean, he could come up with an idea a minute.

VR: [17:00]: Let's talk about Jim. Go on, talk about him, please.

BC: Jim was an idea a minute, but we always liked to guess that, when we were writing papers, when we were doing things, when we were exchanging manuscripts, it was

always, "Did you ever get that from Sedell?" and "Did he ever send that back?" or "Did he ever send you that paper he promised" and most likely the answer was no. But what we came to learn, was that was almost a symptom of Oregon State. To this day, you (well, to the extent when I quit working) you just stay in, or anybody that seems in that bunch for a reprint or send something, it's just like pulling teeth. But Jim was great for ideas, keeping the bunch loose, getting back in focus if we'd drift. He was good at that. Very good.

VR [18:00]: How about Wayne?

BC: Wayne, without doubt the Continuum would never have succeeded without him. Once Robin gave us the idea, the show was essentially Wayne's in my book. He was the guy that made the papers go through, the logistics, it's just that simple. Robin gave us the idea. The rest of us fleshed out little different parts, and then it was almost like, it's Wayne's show. He was the Bear, we called him. He made it go. There's a lot of things I don't think it would've ever gotten done without Wayne.

VR [18:47]: And then of course, this is the most difficult. What do you think you brought as a contributor?

BC: I will not try to fool you. I brought less than I think anybody else on this team. And it was partly because the main reason I was involved at first was because, as Jim mentioned yesterday, the radio tracer aspect of it. And we thought there was going to be a place where radionuclides could answer a lot of questions with whatever we decided to do. Whether it's nutrient tracing or whatever, there was a whole aspect that I can bring that to it. Well, when that didn't enter into it, let's face it, that aspect gone, I had much less to bring than Wayne or Kim that had straight research careers in stream ecology where mine was more concentrated on the cycling of radionuclides in streams. But not so much of the total stream. So, quite honestly, I did do a lot in helping papers through. Wayne sent me lots of manuscripts to look at which the other guys just never returned. And then they also, during the first initial part of all the data collection, I was in charge of all the chemical tabulation in a bunch of this stuff. I brought some to it, but certainly on a scale of 1 to 5, I was definitely a 5.

VR [20:23]: Well, I think if you listen to the other contributions, you'll hear that you're underestimating what you did.

BC: Well, I hope so.

VR [20:31]: So, thank you. Now, do you think the people outside the Continuum...Do you think they realized what roles different people were playing, or did they view you as kind of this conglomerate?

BC: I had ups and downs with that. Quite honestly. I think, and I'm not trying to be egotistical here, I think my stature leading up to being elected president of NABS was built entirely on the Continuum and association with these guys and these relationships,

which says to me, we were viewed pretty highly as a team. I also know in my own heart, that somebody, maybe like Stuart who reviewed it during the program and was very close to it, they could see the major players and that I was kind of a helper with Wayne. So, I think there were some who could see it both ways. But the bigger school I think, saw us as a group.

VR [21:43]: Were there any ways or directions that you wished the Continuum went in, that they didn't go?

BC: Yeah, I wish they had gone for more work where I could've been more integral with the radionuclide expertise. I wish I could've contributed more. Surprising thing is, that came after the Continuum. When we wanted to get to this FPOM transport business, Wayne and I wrestled, the history of this is kind of interesting and it probably has been reviewed for it. We wanted to find a way to trace FPOM. We wrote a proposal to NSF and we said, essentially, "We want to develop a tracer. A way to trace particles. Ground-up walnut shells and this and that and then here's the research we're going to do." It got bounced because they said, "Don't come to us with a method development. Give us the research and don't worry so much about the tracer." So we re-wrote it, emphasizing the research. This one came back saying, "Don't give us the research until you know how you're going to do it." So it flunked twice for contrasting reasons. But just about that time, we ran into this paper of these guys in North Carolina. Wolfenbarger and Crosby and we found a way to label the tracer particles with C14. And there was our answer, right there. So after a lot of correspondence with them, we developed and proved we could do it. I was finally able to use radionuclide tracer technology as part of the Continuum. Yeah, if it wanted to go a different way, I wish there was a way to use that expertise more directly from the start. It just didn't happen and there are a lot of reasons.

VR [23:41]: There have been a lot of spin-offs of the River Continuum. Serial Continuity Hypothesis comes to mind. They're modifications of the River Continuum concept. How did you feel about the fact that these came about with a name change?

BC: That didn't bother me at all, really. It said a couple things to me. 1: People were paying attention to what we did. 2: They thought enough of it to try to find a way to hang their own individual hat on it. And if it was by a little minor name change, so be it. The point was, in effect, they were honoring what we did. That it was worth following up on and looking at a different aspect of it than just trying to replicate the same thing in their streams. They were trying to say, "Hey, this is a neat concept. This is good stuff. How can we tweak it or look at it a little differently?" To me, that's a recognition of what you've done.

VR [24:46]: The River Continuum has been used in management and other research and testing hypotheses. Are you pleased with the way it's gone?

BC: By and large, yes. It's only when it's been taken almost as a negative and it's saying, "You can't fit things into a patterns like this for management purposes because we're

looking at discrete reaches and things like this, so don't give me this whole stream thing." So, that, in effect, that I haven't been involved enough in management to really probably answer this whole question for you, Vince. I guess another way to answer that is I'm not sure that management has yet recognized it for what it is and what it can do for them. I think it's still looked upon as theoretical and "Don't bother us with theory". Now I sound like Battelle. Pardon the sour grapes. In some ways it seems it's too theoretical for their own practical uses and they don't seem to be able to adapt to that. That's about as close as I can...

VR [26:07]: You've had a great 40 year career. If you look back at it and you think of what are the main contributions you've made and what's been your greatest satisfaction, your greatest personal satisfaction?

BC: My greatest personal satisfaction is that when first asked to be in the Continuum and my association with that. That was by far the most intellectually stimulating piece of work and bunch of people to be associated with. Ever. I like to think that in the radioecology field--and this may come out of the little sour grapes--because I've always felt that the field of radioecology in general is been underestimated and underused by ecologists. The hey day of that field was probably from the late '40s till the late '60s. But there were four symposia produced. There was a ton of neat ecological work done with radionucleis, but to me it was largely ignored by pure ecologists as being, "Oh they're just looking at contamination and major numbers." But I think if you dig back into the contributions that that field made to this rite I'm saying to sum up my satisfaction and I think I did good work. Some of that tracing and uptake of the different things like zinc 65, by macrophytes and some things like this. This was important stuff and I think it was good stuff, but I just don't think it was ever picked up and realized by the rest of the ecological community. But I was satisfied doing with it what I did with it. I'm perfectly happy with it, so I guess that's all I can say.

VR [28:03]: Thank you very very much.

Jim Sedell Interview

VR: Jim, thank you for being here with us. Tell us a little about your early life, and especially how you got interested in aquatic ecology.

JS: I was conceived on the banks of the Rogue River in Southern Oregon, and my dad liked to fish, and so we—and I grew up in central Oregon on the ?Shoots? and the ?Matowis?. So learning to fly fish was probably my earliest recollection of streams and how they were a little different and what not, and the bugs. And I was more interested in catching fish, but still, it was real formative. And then I would dam up little streams, and my idea of a good time was to walk from ?Redmann? to the ?Pashoots? River, play around in the side channels, cook marshmallows and hot dogs over a fire, and walk five miles back to town. My brother and I did that from when we were ten until we were about fourteen and moved from Redmann. At that time you didn't think about little kids out on a highway. I mean, you just did it. So I got to college and I was a philosophy, physical science major. I was going to be a lawyer. My junior year one of my good friends walked the beach and he could tell me all about these algae and all these little limpets and snails and critters and I thought, "Boy, that beats the hell out of a law library." And so I applied to a whole bunch of places, and I'd never seen the east coast. Pittsburgh accepted me, and their chairman at the time was from a little liberal arts college like I came from, and he wanted to see more of that there. So I got there, and that's where I met Ken Cummins and became—and really got interested seriously professionally in streams because of Ken. And so I became his student, and by the time he got through all these ?comps? and orals and all that he ups and leaves for Michigan State. I wasn't going to go through all that stuff again, so I stayed there and worked with Bill ?Koffman?, who had come back from a post-doc in Germany. So I finished up at University of Pittsburgh, but in the meantime I kept in very good touch with Ken, and went to ?Gull Lake? To process my work. So I stayed very closely touched with ?Wetzel? and ?Cummins? That really launched me on a track. And then I applied for jobs, and Jim Hall and Norma Anderson were looking for someone to ramrod the International Biological Program stream and aquatic stuff. So I applied and I got that. Then quickly—but before I went there, I said, "Well I need to go meet some people that are doing interesting watershed stuff", and one of those was ?Stuart Fisher?. And the other one was ?Robin Binot? And so just as I'd finished and before I headed west I went up and spent three days with Stuart there at Amherst. And a very generous and very gracious—cause I said, "Well I think I'd like to try to do something like you're doing, only on a coniferous forest in the West." And so we talked a bunch about that and pitfalls. And then I went to ?Stroud? And that was a real eye-opener. I mean these people had all these gizmos at the time, and knew all about these bugs, and were taking it more from a community college-y approach, very different from what Stuart had done. And it was just very exciting there. And Rob would kinda do his thing, and all the time he was smoking a pipe, and would walk the stream and wave his arms, and it was real exciting. And so then I came back to Oregon at Oregon state, and what got me was that we had all these parts, but we didn't have a story. We didn't hang within a coniferous forest ?biome?. We had a fish story, we had an insect story, and that. And so Stuart's stuff, we just said "Okay, I'm going to find a little watershed ?ten? and we're going to a

carbon budget. And at least that'll connect us to the forest. And that'll be a place to start. And that'll be something systemically that we can hang a lot of the community work that Norm and Jim did. And at the time, ?Jack Lyford? Was into mosses. And so that's what got started. And probably the best thing that happened was that both Jim and Norm went on sabbatical, because in the time they were on sabbatical, then I could work with ?Jerry Franklin? And ?Dick Werren? we totally rearranged what we were doing. And then that sustained itself through a long time. We got ?Frank Triska? There and did a lot of stuff. But that's how I really got into it, was fly fishing and walking and chasing crawdads in the "Shoots River".

5:02

VR: You've certainly had a broad professional career, in terms of working at different places, and you've been in industry, you've been in academia, you've been in government. How did these influences affect how you view streams and stream management, especially?

JS: Well I think the early ?dear the katims? Going back to Pittsburgh, certainly opened my eyes to deciduous Eastern forest streams. Because they were very different from anything I'd seen. I was used to mountain streams in the west. And white water, pretty pristine. And these were highly developed agricultural streams, or highly urbanized streams. And so it was a real eye-opener. And I'd looked for ?wreckage? And when in Pennsylvania we'd go to streams in ?Heart's Content? Or ?Crook's Forest? And see how these remnant old white pine forests might interact. And that was all kind of neat, and like I say, the ?node? and ?Strautzen? really opened my eyes to a whole bunch of other possibilities, and Ken was into his ?trophic? dynamics and the ?gills? So that got me going. And when we got back, that whole science part got me thinking systems with the IBP, with the modelers, the nutrient cyclers. And Oregon State had made a tremendous effort. They decided their future would be with post-docs. And so I was one of those. And ?Fred Swanson? Was one, and "Frank Triska?. ?Kermit Cromac?, ?Check Grier?, and a bunch of those folks have gone on and done really nice things, and then ?Stan Gregory? Was there as a student. I spent the first six months trying to get rid of ?Stan Gregory?. And it was the best thing that ever happened to me, and surely to the program that we never did. I tell that just out of fairness, because he was taking money that I needed. And he was at the time doing something very different. And we laugh about the relevance of what he was doing now. So that was it. And then the continuum came along. And that opened our eyes to start thinking bigger about comparisons, along different ?size?, about a gradient of streams. A tremendously exciting time. If science is about discovery and creativity, both those experiences were it. But the continuum more so. That truly was an up-from-the-ashes creative effort that everyone then started to contribute both to the robustness of the ideas and to the execution of trying to get it to start. I had some personal family problems that really stemmed from the continuum, and so I dropped out, and I needed to get a bigger income to support myself and my family. And that's when I went to ?Werehauser? And at the time, it was a shock to everyone at Oregon State that I left academia and went into industry. But I needed, for very real, financial reasons. But the thing that got me into touch with was ?Pete Bisson? And all

this industrial-force land that had cut over. And really the very best most productive land in the West was private industry land. So I saw things I'd never seen before, and I could then start to look at the extremes of the disturbance. And that got me thinking about integrating some of this theory into the management and the importance of some of the early stuff we were doing into that. And then with government it became apparent I wasn't really an industry biologist at heart. The industry couldn't decide whether they really wanted a scientist, in which we had certain rules to play by, of transparency and give-and-take, and open debate, versus a gladiator, in which you hire a consultant to advocate technically a point of view for them. And what brought it to a head was they didn't want me to publish the whole story. They liked this part of the story. And I said, "Wait. Either publish the whole thing, we'll put you in jail, and you have a science Cadray, or you don't." And so that drove "Werehauser" after I left to allow this and then "Bilby" who replaced me to publish stuff that I am sure those execs never bothered reading but are just amazing, very very good science papers at all. And then that got me into the riparian area. Cause that was what was by the industry. What was the role of riparian areas around streams. And we'd been looking at the role of forests and bigger streams in that. And then when I went to the forest service. That started to get me back with Fred Swanson. For one, and started thinking more about geomorphology and then applying some of the basic sciences to what was fairly much a fish habitat program. And while my slant then still continued to still be on the fish habitat, it was more towards the riparian end in the basic science into it. But when I joined the forest service was in the same year that Mount St. Helens exploded. And that allowed me to coordinate the aquatic work around St. Helens. And we brought in a bunch of oceanographers. Some had been in origin of life discussions--John Barris. And they were very much into these primitive archy-bactors. And then anaerobic things. And I know that we would never have looked at that if I hadn't been around the Rick Larsons and the Klugs and other folks and Tom Bot. During the continuum. And there was amazing, wildly, exciting time. But that got into disturbance at still another extreme. And then how do you recover from that disturbance? And then, like I was saying earlier in the interview, that really everyone said, "Well large wood only plays a part in really small streams". And I felt like, no, wherever there was a forest at a bank, a river, it had to interact in some way, and I could show it all the way through the Mississippi. European rivers went a couple years before ours. So I started to compile historical reconstructions of rivers. The Polammet river is probably the best of what I did. And then splash dams. Just history of how rivers got there, and how what we were seeing wasn't what the final end point might be even though the water looked clear. It had been really changed.

12:00

VR: How did you get involved in the river continuum.

JS: Well I think a lot of it from my point of view was because I was groping for some overarching way to approach streams in the biome. And at Oregon State there you had Charles Warren, and I couldn't understand him. I mean, it's not that I disliked Charles at all. I didn't. It was just that intellectually, I couldn't figure out what he was doing. And then Resnet I couldn't translate it over into the exciting things that Fred

Swanson? And ?Jeret Franklin? And all those students were doing. And so we brought in a whole bunch of people to help me sort through what was—to see if he had something that I could grab onto, and if not we could interface him. And we started to ?cook? about a lot of ideas after we'd come out and shake our head that we couldn't understand what was going on in the room before. And that started to get things going, and through various workshops of which probably only ?Burt Cushing? Is recorded. We started to get together. And that's when I went—Jerry Franklin left as coordinator of the coniferous forest biome and took a stint for a year or two as a program director in ecology at NSF. So I called Jerry and I said, “Hey, I think that if we're interested in stream groups getting together around the country, which was part of the inner biome comparisons. There's a good group of people. What about a grant for looking and seeing that there's something there.” And so we put a small proposal together and it got funded by the National Science Foundation to meet to start to discuss, and I think the others might have said we floundered around and whatnot until we had a meeting there at either ?Stroud? or ?Kellogg?. I forget which one. Suddenly Robin started waving his arms and letting it go. We all knew that he had a story, and we couldn't pull it from him. And he was really careful at guarding it, and finally he just let it loose. And it was a great story. I mean, that was the foundation. And then we just went. We just knew we had to do something, and that gave us the framework to hang up a clothesline.

14:32

VR: When you finally did start working in the RCC, was this the main focus of your research, or were you doing a lot of other things.

JS: No, I was doing a lot of other things. My job in a lot of things was to get the money and keep the crews going. And I would write the proposals for the aquatic stuff for the ?at? We hired ?Frank Triske? We had ?Stan Gregory? On. We had two or three technicians going there. So we were doing a carbon nitrogen budget. And we were doing all the stock stuff, leaf packs, I think we published those. And so I was keeping that going, and then when the river continuum came on, then I started sliding that onto Frank and to ?Jim Hall? And really stand to kind of finish up and I moved over to a rut in the RCC. But I still had these biome responsibilities at the time.

15:37

VR: Let me ask you about the different participants in the river continuum concept. What was their role, and what do you think the main thing that they focused on, or they contributed. Let's put it that way. How about ?Robin Benot?

JS: Oh, ?Benot? was certainly the creator of the concept, and he'd been thinking about it through his bugs and temperature and that. But also he's one of the best naturalists we had. He's curious about forests. He'd ask questions about history. He'd thought about well, what was the view of a stream from a horse's back, cause that's how the early descriptions of streams were, versus walking. You know, it never dawned on me that being five feet higher would give me a different—I mean it would, but I wasn't thinking

about that. He was thinking about just little things. So he was very curious about the world. And very creative. So he had that idea, and at the same time he had a certain amount of rigor, particularly as he approached the bugs. He had such a wealth of work with muscles and ?TVA? and had been on all these channelization studies, and so he'd really seen a lot of things going around. And particularly he knew farm, these woodlot farmland streams. Very different from anything I'd read with the ?Solar? conservation servicer people that played with that. So he definitely was to me the creative leader of the bunch, and I really resonated very well with Rob, when we could wave our arms, and it was just a joy to be around. Just a real joy.

17:26

VR: How about ?Burt Cushing?

JS: Burt Cushing was probably one of our more unsung members up there because he was from a high overhead place in ?Battel? But he had a lot of experience with the desert biome and then at ?Battel? working with desert spring streams. And he knew a lot about isotope use and chambers, but his strength was a good follow through. He would do what he said he was going to do, and in retrospect, he was essential for ?Mitchel? maintaining his sanity. Mitchel would get exasperated with Robin and I because we didn't follow through or we wouldn't buckle down and do things. And so Burt in my opinion played a key role as a ?brokering? when relationships were strained and he'd kind of work in on his own way, but a lot of it was with Ken and with Wayne, keeping them in the game and going. And like I said, he's technically no slouch. So he was really essential. And then in the after part, when essentially Robin and I weren't doing a whole lot, he was essential in the follow-through, for getting things done with Wayne. That's the way I saw it. Plus he's just one of the great gentle people in aquatic ecology. He had a huge network, and he cultivated in it. And it wasn't through science expertise. He understood science, but he felt that personal relationships were real important. And he still does. I mean that's, to me, why people still go to him.

19:31

VR: What about ?Ken Cummins?

JS: ?Ken Cummins? Was probably the best known of the stream ecologists of all of us. Certainly the world, only a certain amount of insiders knew about Rob and I, I think. But Ken Cummins had come out with some ?trophic? levels, and had a number of workshops ?there a time at tuning? And the energetics book he did, and so he was really climbing. And he had a lot of great ideas about insects. Again, he's no slouch as a naturalist. And again, I look at him as the first mentor I had in streams. And so he brought a maturing idea of streams, still bug-centric and leaf-pack-centric at the time. And then I think really while we kind of beat up a little bit on ?Wetzel?, ?Wetzel's? look at C-14's and carbon cycling in the lakes helped influence some of the technique's development, certainly that they were using at the lab there at ?Cull Lake with the internash? But he brought a

credibility to us, and a follow-through. And he had a lot of ideas, also. So he was a main player.

20:57

VR: The fact that you had been a student. Was that ever awkward.

JS: No! Because we kind of made this split. And he'd gone to Michigan State. And he'd kept in touch, and again a very generous a person. He'd open his lab. He and ?Bob Peterson? put me up. Cause I didn't have any money. And I'd go there for several days and I would use ?Bob Wetzel?'s equipment to do it. And so again, very generous, very open, very open with ideas, didn't hide anything. So you always felt like you were being treated with an equal respect. And then when I got out to Oregon State, you could start to carve out a little space of your own. And I felt like I had space of my own. I was doing things that weren't in competition. In fact, a lot of what I did early on was just a copycat of what ?Stuart Fisher? Had done, or the leaf-pack stuff, only we'd use needle-packs, and we'd look at it a little differently, but the same thing. And so I felt like, okay, that was a way to get started in the field there, and he was very important. And then I think that what happened was we got him out on a sabbatical in the early days of the continuum and then eventually he came back as a professor there at Oregon State, and that was really a plus. But it never felt awkward, because I wasn't there like ?Bob Peterson? Was, where it was a post-doc and he kind of grew up with his thesis. Cause my thesis got started and then Ken went and really Dan Nelson was my thesis advisor from Oak Ridge. So intellectually, it was a little different spot, even though we could talk about it. So we developed a pretty good relationship, I felt. And still do. Any of these people I feel like I could call in the middle of the night, and they'd grumble and roar at me, but they'd open the door.

23:01

VR: What about ?Wayne Mitchell's? contributions?

JS: Oh, Wayne Mitchell—The Bear. In any science project of this scale, you need a variety of people. And at the start you need the idea generators and that, and then you need the implementers and the data collectors and that, and analysts, and then you need the synthetic, the synthesis people at the end. They aren't always the same people. And so Wayne was a good thing cause he could keep us honest. He was the Lorax of the river continuum. If Robin came up with the idea, Wayne spoke for the concept. And he spoke because he followed through on the papers, and he controlled and beat up and yelled and got angry with us, and really was the general that bashed heads to get the paper out of them, and if people didn't want to pay he'd just go ahead and do it. And so that was an unfair burden. He didn't ask or aspire to that. But by sheer force of his personality, and he was inclined to analyze and get it out, he did. And so I would consider him the Lorax of the continuum. I mean, we would have had that. The Lorax spoke for the trees, and Wayne spoke for the concept. And he did that in his writing. He did that when we had to do a defense of something with like the paper in the Canadian journal where a ?Statzner?

and ?Higgler? and ?Weirnar? He had to organize and galvanize the forces to go do it. And it's reflected. I think that if you looked at symposium around Europe and that, they weren't calling on Robin or anyone else. They were calling on Wayne. And so Wayne would go over to the British Ecological Society, or Wayne would go to some of these other meetings, and really represent us. And I think ten years or so after, he would get a tag, and unfairly in many ways, of being very defensive about the continuum. He was really on the front lines on the international banquet circuit, so to speak, defending this, and looking at the nuances and that. So to me, we wouldn't be here talking to you if it hadn't been for ?Mitchell? And so he was really important. The other one I'd like to talk about is Peterson. Peterson and I started graduate school together at Pittsburg. And then when Ken left and went to Michigan state, he went to Michigan State too. And he was always a—Milo the Bull, we always called him. Very strong, physically strong person, very intense person in personality. But he really liked technique and methods. And he was a good experimentalist. And when Ken would tell you about something, Bob would think about experiments that could test this. So he was an excellent experimentalist. But he really had a flare—he liked the statistics. He liked the analytical part. Well that was not—So one of these meetings where we had them all out and he came out and where I got crossways really with Bob was we were up drinking beer and I was showing him the work in the ?Andrews? forest and what we were thinking and trying to get his ideas on how we would proceed there. And he just made a comment, and probably he didn't even remember the comment, cause he'd had a beer too, that I would never amount to anything in science. And that pissed me off. And so then when he went on to Sweden and post-doc-ed there. Then they said they were going to bring him on as a post-doc. I objected. But then we decided that there wasn't anybody better to do that, so we'd do it. So there was always a kind of a clash of the egos there. And we did it both physically, when we'd wrestle, and verbally. So there was a tension that Wayne, and Ken, and Robin were always—and he was far better analytical skills than I ever had or ever aspired to have. I mean, he was essential to knowing that he needed data and how to go about analyzing it. So we kind of split apart. And part of it is when you're into data sheets and a rigid analysis, you couldn't see this rich tapestry we were trying to describe with this really simple river continuum. And that always frustrated me. That we needed to make some very real but narrow choices to describe what we were doing and then go do it, and to me that wasn't all there was. The richness to me and the excitement was seeing all this variability, and then how do you accommodate that. So it was the intellectual excitement that stimulated. And he was rightly so went after the product. And it wasn't that he didn't think about those ideas and didn't have good ideas. That wasn't the point. We separated and we really didn't talk to one another. And then at a ?Dom? conference ?Cliff Dom? Said, "This is nuts". He'd gone to Sweden, and he knew there was tension between Bob and I. He said, "Jim, this is nuts. You have to bury the hatchet, let go of things." So we met and had dinner, and all this was about ten years later, and buried the hatchet. But there was still some ??? And then he came back to the US and had his aneurism and was in the hospital. And they were trying to decide whether to send him back to Sweden or it was more advantageous for the rehabilitation to be in the states. So I made a special trip there. And I called ?Marsof? and others to see if he would take visitors and all and he said, "Yeah, he's kind of depressed. It would be nice to have that." So I walked in and the nurses had changed and they said, "Well who are you? We only

allow family in.” Well I said, “I’m his brother.” And walked into the place. And they led him into the room, and from what I gather afterwards, from talking to Ken and others, he was very lucid. And for two hours, he was very lucid. And he looked at me, and I introduced myself formally. I didn’t know whether he would recognize me or not. And he says, “Ah, Jim Sedell. You always show up at the most interesting times.” And that kind of set the tone for what I thought was for me a major closure with something that had ruptured. Because I used to stay at his house, and we got his cat, a Siamese cat, things like that. So that was extremely important, to come to closure with another main player in the continuum. So lots of human relations, and not just science that goes into these things, I think.

30:45

VR: Do you think that people outside the continuum understood the roles that each of you played?

JS: I think they did in some ways. I mean I could see that I was energetic and waved my arms a lot. And they appreciated Robin when he got up. And I think that as time went on they got a much clearer view of who stayed engaged and who followed through. And then there are certain crowds that you play to. I mean, Robin in the northeast and the east that may not have appreciated what the folks in the west did. And we in the west played it towards where we were. But I think the science community has got it pretty figured out now, as to what the kinds of roles that were. But they might have known in the beginning.

31:42

VR: Were there some directions you would have liked to have seen the RCC go into? I know you left it early. But were there some things that really should have been done that weren’t done or directions that should have gone in that weren’t?

JS: Oh, yeah. And I think it got into that one paper where we showed the canyon regions and the different ridge types that could explain some different kinds of processes. And I would have liked to have seen us take a little stronger look geomorphically. And we were heading towards some of that. And again, Robin really resonated also with Fred Swanson. And Fred Swanson had a really great view. In fact a lot of his both spatial and temporal scale stuff? Christopher Zell? Formalized. But that really came from Fred’s geomorphology class at Oregon State. And so I would’ve liked to have followed up on some of that. I was less interested in the bugs at that time, even though I got started in aquatic insects. Ken and Robin were really into that. So I was into taking a look at the bigger picture, looking at the geomorphology and some of the transport kind of things.

32:59

VR: Large spin-offs of the river continuum like the serial discontinuity hypothesis and some of the others were really modifications of the river continuum concept. Did it bother you that they took the basic idea and they changed the name.

JS: Well, actually no. I mean, I would get exasperated with some of the literalness that was going on. But I felt, whoa! You know? People are responding. And if anything we wakened some creative juices in a whole bunch of other different people. And they were scrambling for taking their real life experience and trying to explain it in a little broader context than their papers had indicated before that. So the way I looked at it, it was more satisfying that. Hey, we knew we had holes and limitations in what we did. So now people were trying to shore it up and see what they could do. But I didn't think any of them ever got to that overall picture, that world view, that we laid out very simply. They took a piece of it and explained it. And some explained it a lot better than us. And I guess that's what science is all about. Stimulating ideas and people follow through and take little different attacks. And maybe we won, maybe we didn't. Maybe we're going to change it. We just change our fairytale.

34:33

VR: Were you pleased with the way it's been used in management and application?

JS: Well, it's often cited in major regional assessments and EISes. But it's really limited as to how you'd use it in a management sense. Cause a management sense is all, other than if it's not working and it should be working. Like there are no trees where there should be trees. You've got a problem. And so you could use it to explain the foundations. It was extremely important in explaining to managers why very robust ?riparian? forests in ?riparian? areas were important to streams. Not just to salmon, but to a healthy river down where it went by a town or something. And until then we didn't have that. We were really just focused in on a site where a logging project or a ?show? or something like that. But this allowed us to talk about the whole network. And then you could see that big streams still needed something. And little streams, you could argue, needed even more because they were a lot more intimately connected to the stream than the bigger stream. And so I think that gave us a framework. I don't think it changed someone's mind into leaving a hundred-foot buffer or a two-hundred-foot buffer. But it allowed us to hang what we were trying to do with an explicit thing to a framework of how a stream or river operated. If we wanted to sustain it, we had to keep it that way.

36:20

VR: This is going to be a tough question. You've had really a remarkable career. If you look back over the last thirty-five years, what do you think your greatest contribution as you leave will be?

JS: Well, yeah. What may be what is left may not be what I'm most proud of. In the sense that I think that the continuum certainly ranks as one of my science achievements. I think the IBP work with ?Frank Triske? And ?Stan Gregory? In the old biome that got

the stream team started at Oregon State was a real highlight. And I think that those small-stream studies, that then we rolled into the continuum. Those'd be it. So the getting together with the ?Jerry Franklins? And the ?Stan gregorys? And the ?Fred Swansons? Were really seminal and important parts of my development, that set me up to be an active and contributing participant in the river continuum project. And after that, I think the historical reconstruction work, and the work with large wood. Because nobody was seeing that, and people said, "Well, my job is to go out and publish papers." Well I didn't publish a whole lot of papers. But there wasn't a garden club or a civic group that I didn't tell a story to about large wood and the need for big trees and ?mercinal? trees. And so there was a two year period when I must have given a hundred and twenty talks. A couple a week, on a single story, that we needed to manage streams differently. And that culminated in the Northwest Forest Plan. And in terms of translating science into an ecosystem vision for restoration or sustaining it that was put down in law, that was laid out and saying that's the best translation of science that we can do for ecosystem management at the time. And that was the culmination of the work in the river continuum and the old biome and said, this is what you do if our mission is to sustain fully-functioning streams and rivers in the northwest. And this is kind of what we have to do. And I think that those would rate. And then the historical stuff would point out that when we had been ?priming? streams forever and pulling the wood out of them, and that's just a ?passion? cause I could do that at any library. And I would go and give a talk at Utah State, and I'd go find out who'd done work on the ?Cache? river, and see what it looked like. And it was always the same story. There were a lot of trees. There weren't trees now. There's evidence of wood and/or beaver. And so those'd be my greatest ones. And if I finish out in the legacy, I'd like to finish out with the establishment of some experimental forests, where people in the future would have a chance to do some manipulation of that. But I feel like I've been very blessed and very lucky to have crossed paths and maintained common courses with so many wonderful and bright people.

40:05

VR: Is there some aspect that's given you the most personal satisfaction?

JS: Well I like the history part. I mean personally, I thought that was it. And it always tickles me. ?Stan Gregory's? really doing a wonderful peace of work with ?Dave Folse? On the ?Wilamette? and they remeasured the ?Wilamette? stuff, and I'd done it with just ?Judy Froget? And I'd just done it with a map wheel. And we came out very close when they ?GIS?'ed it. And all-referenced it. The storyline didn't change. Some of the numbers changed a bit as it got more precise. And I felt very good that we didn't blow it with our primitive tools. But I thought the history stuff there, and then I guess ?St. Helen's?, but that was more what it did for me in terms of seeing a cataclysmic disturbance, and nothing that I could have ever thought about or envisioned. And then now twenty-five years later, seeing some incredible recovery and changes as it was reset and now it's starting back.

VR: Thank you very much.

Interview with Luna Leopold
Tuesday January 11, 2005 2:00 PM
Location: Luna Leopold's house

VR: Ok, so we'll, after our false start with the...

LL: Well, before you come back, there are three things I would recommend you look at that are not listed so far. Langbein and I wrote two papers to try to explain, in simply different words the whole concept, and I gave you those references to look at. And the third is I wrote a whole book about it, and I think you oughta read that. Well, now, the problem is basically this. Robin Vannote took my idea whole hawk and said "Geez that's a wonderful idea, it must apply to the biota." Swell. But if he'd called me, which he didn't, and I knew him very well, I'd have said "Look Robin, you're starting in a very difficult direction, because you've got a theory with no proof, and my suggestion is if you're going write such a paper, lay out, at least in principle, the kinds of investigations that you think oughta be made in order to start approaching the idea that this actually works in the biota." But the fact is that he published the idea without even suggesting how it might be verified. It took me fifteen years to develop that, and it was built step by step by making certain observations, recording them, and then seeing how they're going towards a whole. We didn't even see the end of it until 15 years had gone by. Therefore, the problem that Vannote got into is that he jumped the gun, in effect. Now, if you sat down and looked at the idea, that these interactions that occur all are stochastic around some kind of a mean, and that mean means most probable state. That being the case then, we should start out by saying, let us just take some examples in the biologic field. For example, the continuum idea which I started in 1954 which started with the downstream hydraulic geometry meant that things moved in the downstream direction, and there was a progression in the downstream direction. So, what might be the progression? For example, and I haven't thought these through in such detail that it's a workable system, the principle of biota in the stream, let's say of the insects on the stream rocks, ought to change progressively in keeping with some parameters that the river is dictating. Now what he mentions in the original paper, what the feeders on the organic material that's flowing downstream is feeding on the leakage. Is that the word he used?

VR: Mm-hmm...

LL: Well that's crazy, because it's not leakage at all. It's simply that there's more organic material available than is utilized by the organisms that are eating it. That's a very misconception, to label it that way. So, how would you measure this? Well, you would probably start out by saying "Let us take a certain variety of organisms, and we will start defining their distribution in number and size or in some other way, and see how that changed downstream." Or, some other way, or let's talk about the so-called leakage. Let's talk about how the excess organic material, which is food for the microorganisms, how does it change in variety and in mass downstream. Or, you might say "Let us talk about the total available mass downstream." None of those things were mentioned. And the thing is, that when he, as I understand it, but I never heard anything about it other than reading it, they went on this expedition up in Idaho, and the thing was

a complete bust, because they had no idea what the hell to do. And they therefore didn't do anything very constructive. And so after that, the whole thing dropped. It was a shame. So I would caution any scientist to be careful about starting with a hypothesis, and publishing the hypothesis without having the slightest idea how you're going to go about saying how it worked, and I think that's the problem that you ran into. Considering the fact that it took me a whole book to describe, one by one, all the features that went into this theory and how they all fit in together to make it a whole, and how the final breakthrough came about. And that's quite messy, you see, in the business of the stream biota. So, anyway, that's the gist of it the way I see it.

VR: How did you know Vannote? Was it through Ruth Patrick?

LL: Well I used to give a short course at the Stroud Laboratory for years, when he was director. I knew him very well. So I was really surprised when this came out in print, he never talked to me about it. I would have said "Wait a minute! Wait a minute, now. Take it easy. I agree with you, BUT..." Then of course, for reasons which I never did understand, Robin became very unstable after that. I don't know why, I don't know anything about the history, but I knew though, that there was a tendency, I felt, as indicated by this matter, for him to get ahead of himself. Which was too bad, because he was a very smart guy. So, what happened after that, I don't know, because he dropped out of science for quite a while, I'm told. I don't know. It was a good scientist gone awry, but I don't know why. There was something going on that we couldn't see, apparently.

VR: There was a large NSF grant on the River Continuum...

LL: Yes! There was indeed. And when I heard about this, I had submitted to NSF a very small grant, and when I heard about this, they turned my grant down. And I wrote to them and said "You took my idea, and gave a large amount of money." I said "Don't ever send me anything again, *in my life*." And they never did. I was very angry about it.

VR: You never saw a draft of the paper before?

LL: No.

VR: They didn't circulate that with you? Why do you think it's been so popular in terms of ...as a paper?

LL: Because it's a good idea! Of course it's a good idea! But how the hell do you get to a good idea? A good idea came from hard work! Read my book and see how many ways we went at it. Matter of fact, we didn't even put it all together until one instance in which I made an observation in the field that I had never expected, and no one had ever expected. And all of a sudden, I began to see what was happening. There was a *balance* between things. This was giving up something for that. And we had never seen this before, and that led to a whole lot of new things. So, try as we may, we simply chanced onto something because of an experiment I was running. It never occurred to me that this

was going to be a result. But then it took years after that to develop fully how this thing was gonna go. But my whole book is devoted to the whole question of how entropy operates in these systems, and why we're talking about probabilities, and the most logical state, the most reasonable state with the least variance. It was a long long process, and that's what Robin was simply avoiding, which I think was too bad. Good idea? Yes. Good idea.

VR: When you were doing this book, and you were thinking about this issue of entropy, were you thinking of it as having management implications, or more a theoretical explanation of how rivers worked?

LL: Both...yeah. Because...well that's a whole 'nother kettle of fish. It has management implications in this regard: that there aren't definite rules with definite outcomes. And any management has to realize that that's true. You're going to have deviations, which are natural. They're not deviations being wrong, they're simply deviations from the mean. Management doesn't usually look at it this way. They say "this must be so, and therefore that must be so," and it doesn't work that way, as we all know in biology, it just doesn't work that way. So, there are lots of other ways in which management is affected by this, but I'd have to think over one at a time the kind of things that might be affected. But management, yes. Yes indeed. What do you think?

VR: Well, I think it's certainly been talked about being used for management...

LL: But you see, they don't know how to use it because they haven't explored what this means in terms of the biota. It certainly is perfectly clear in terms of the physical system. Perfectly clear.

VR: Let me ask you. Can biologists and hydrologists interact together successfully? There's been a lot of talk about this happening. The AGU in the spring meeting will be meeting with biologists.

LL: Why don't you look at the last paper that McBain and Trush and I wrote. There's exactly the answer to your question. The most important thing that Trush and McBain did up there on the Trinity was to demonstrate in detail how the vegetation, the riparian corridor, interacted with the stream. And we put this together in a paper, that we published at the National Academy, which I think is the most important paper along this line in the whole business. And it's the first time that there has been a real integration of the hydrology part together with the biologic implications part, in one simple statement, because it involves these things. First, every part of the runoff cycle has got implications for the life biota. The high flow in the spring, the low flow in the summer, the medium flow at other times. The five year flood, the two year flood, the twenty-five year flood. Each one of those things has a specific relationship to what the biota need and operate on. There are many many examples of this. For example, that young man with the Italian name that I can't remember wrote a paper, a very good paper, a long paper, on the cottonwood grove that makes the beauty of Jackson Hole. He pointed out that because the floods had been cut off by the dam, these trees are going to decay, and there's gonna

be no reproduction, and Jackson Hole is going to be left without any trees. Why? Because the seeds of the cottonwood have to be lifted high enough up above the floodplain itself to be protected against the ordinary flood, which is what McBain and Trush and I have pointed out in that paper. If they are not so protected, you're not going to have any reproduction. Well, here's a very practical outcome of a perfect simple fact. You cut out that important detail of how the seeds get up where they can be protected, and you have no trees. That's a hell of a big thing, because where did Jackson Hole get its reputation? The beautiful corridor of cottonwoods down through Jackson Hole. That's an example of that. Then, we have in that paper...do you know that paper at all?

VR: Yes. I've just...

LL: Alright. Well, anyway, I drew a diagram in which we got all, divided into the various several parts, the effect on vegetation on the fish, on...in other words, separated, but show which part of the ?ramda? wavelength is devoted to each one of these items. This is a very clear explanation of how the environment for the fish is altered by various stages of flow. Anyhow, so there's, in my opinion, an example of just what you say – the collaboration between hydrology and the biologic sciences. I don't think there's a better example that I know of. So I say yes indeed, they can work together. There's lots of reasons why they won't, because of the engineering viewpoint of some of the agencies, but that's sooner or later we're gonna have to overcome.

VR: One of the things that I was looking for in this is the influences on the river continuum, and that's why we came to you first. What influenced your ideas about zonation? Were there any hydrologists working before you that had given you the idea?

LL: No.

VR: So this was all on your fifteen years of observation?

LL: That's right. It started out when I started to work on the reason for the width of the river being as we see it, and I began to see that the river width was connected with all these other things, and that it changed progressively downstream. Then we began to see, in that paper, between what I call the outer station and the downstream. That was the beginning of how the continuity from upstream to downstream is maintained in the physical sense. Everything went from there on.

VR: There was no indication that anyone in the literature had had this idea?

LL: No. No.

VR: I know that you did work in Africa and various areas throughout North America. Did you see much variation how hydrologists or biologists, how our streams are different.

LL: How what?

VR: There's a line that biologists always use when they hear something about a generalization: "But our streams are different." Did you see a lot of differences from geographic areas when you were doing this?

LL: No. The amazing thing is the more you look at streams on other continents, they fall into the same system that we see here in much more detail. I'm not sure if that's answering your question.

VR: Yeah.

LL: You look at the number of things we had to learn. First, meanders occur as hydrodynamic result without banks. They occur in ice, and they occur in the Gulf stream. That's a very important thing to note. Secondly, meanders occur without sediment. Very important things to know. These are different than any things we ever thought. These kinds of things. Have you read my book on this?

VR: Yes.

LL: Well, you get the idea, then.

VR: It's very interesting to note how so much of this was de novo, that it was original, because so often in science we see that people are building on ideas.

LL: Well, we all build on ideas, but this idea was basically my own. This particular idea.

VR: What about Ruth Patrick's role with Vannote and the continuum?

LL: Not at all.

VR: Not at all? So the connection between her and the academy was...

LL: No...no. She was extremely important in setting up the Strode Laboratory. She'd been tremendously important in getting money for the kind of research that interested her. And potent in the business of trying to give guidance to some of these companies like DuPont and also to the Savannah Flat down there. No, she was not interested. She would never concern herself with that. The way I got us into it was that I asked Ruth to give a course to us, which she did. So I brought all my scientists together from the geological survey, and I thought they might be interested, or should be interested, and we all took this course she gave. And so I started the whole business of "Let's get some biologic science going in the survey," and it did very well. It was never very large, and some of the people like Keith Slack actually retired long before the completed publication of all the things they had learned. But nevertheless, it was a very successful beginning.

VR: Did you ever use the phrase "the river as continuum?" Was that one you had used in talks, or putting the word continuum...

LL: (laughs) No, I used the word continuity, that's all. All the time. So, the continuum is just another way of saying it. They simply turned it into a noun instead of the... The word continuity implies not just... but anyhow, there's a slight difference.

VR: We hope that Vannote will come out here. Would you be interested in trying to get the two of you together?

LL: Not particularly. No.

PD: Picking up the paper for the first time, what did you think about it...the River Continuum Project that Vannote did.

LL: I think I told you exactly what I thought. A damn good idea. Except wait a minute now! You're gonna have a hard time presenting the idea without any indication of how you're gonna go about saying...how you're proving it.

PD: He didn't do his homework.

LL: No. I could immediately see that. It was quite clear that...Jumping the gun in science isn't going to do you much good. It doesn't work that way. Now, as a matter of fact, it becomes an albatross around your neck. If you have to...if you're in a position where now you have to prove yourself, but you're not ready to prove yourself, then this is trouble. And I would caution any scientist to be careful of that.

VR: The temptation, you know the big grant money that came their way was a...

LL: Sure! Of course! But you see, if I had been the National Science Foundation, and I looked at that, I would have said "Wait a minute now! That's a great idea, but now what are you gonna do from then on? What are you gonna do with this money?" And they didn't know what they were gonna do with their money. And what happened...nobody's ever been very forthcoming on what happened on that Idaho expedition. As far as I can make out from anything I've ever read or heard, it was a complete flop. They didn't learn anything from it that was useful. Sure, they made some observations, but it didn't tie in anything. Is that true?

VR: Pretty much. Did you ever deal with any of the other members?

LL: No. I've read their stuff, but I never met with them.

VR: Maybe we should turn this off now...

END

Bernhard Statzner Interview

Vince Resh: Ok, so we're talking with Bernhard Statzner. He's the first person we're interviewing regarding the river continuum concept paper. Bernard, would you please tell us a little bit about your background, your education, your positions, etcetera.

Bernhard Statzner: I don't tell you all my life, huh?

VR: No no no...

BS: Just...educated in Germany at the University of Kiel. Interrupting that and going to Central Africa, West Africa, then my PhD in '79. Then I had a position in Karlsruhe as an assistant professor, as a professor at another university in Germany. Quit science to be a manager, and then went to France to Lyon to be a research director. That's what I'm still doing.

VR: Where were you when you were still interested in the river continuum? Where you still at Karlsruhe?

BS: I was at Karlsruhe, yeah.

VR: So, give us the first thoughts that you had when you read the river continuum paper.

BS: I read it as all people did when it appeared, and there were previous papers. Cummins in '74 wrote something, so we were aware that something may happen like that. I really carefully read it when I prepared a trip in particular to the United States to a seminar here. I went to the Netherlands, and we really analyzed it word by word to understand what's in it, and what are the consequences of it. And what we observed at that time, that was in '83, that already, in the United States, or North America, this had so much impact on the orientation in stream research that we thought it might be rash to look closely to it and then just see what we could get out of it.

VR (2:01): Do you think that your and Bert Higler's reaction to the river continuum was typical of Europeans compared to Americans? What do you think the difference was?

BS: I wouldn't say I was typical of Europeans, because what we did was we analyzed that in a way that I think nobody had done before. So, that was what we did.

VR: What were some of the ideas that Europeans had about zonation and the river continuum ideas that Americans didn't have? I know you told me a lot about ?Tilman? Could you just summarize some of the ideas that were prevalent in Europe in the late 1970s?

BS: There were several things going on. We had this strong impact of ?????? on the zonation front. And, the stream zonation is described in this continuum pattern. So basically, that was already a difference in the continuum view and the discontinuum view

in Europe. That view was based on taxonomical structure. And when Bert and I prepared these seminars for our trip to the United States, we took this continuum view and tried to analyze that in the context of the things we considered as important, and that was basically a European view. And, we added a second part to that. There was an analysis of the continuum paper, and the second part to the seminar and later also the publication was that we tried to figure out how these discontinua that exist in the taxonomical structure relate to changes in hydraulics, and that on a worldwide scale. So, it was an evaluation of the theoretical background of the river continuum concept plus our ideas about discontinua in taxonomic structure that relate to physical discontinuum.

VR (4:42): Tell us a little bit about how you and Bert got together to do this seminar. The rationale behind what you were going to do.

BS: You want to hear the truth?

VR: Yes.

BS: At that time, I was a kid and Bert was also wasn't very rich and we had a symposium at Kent in 1983, so we wanted to go there, and then our plan was to travel through the United States to visit different labs. To get this paid, we had to apply for funds to prepare the seminar. And then I got the funding, and Bert didn't. So, I came here in Berkeley for a brief moment. I went to Corvallis to give this paper. And I had a great time, travelling through the United States. That was basically the reason to do that. We didn't do it planning to write a paper. But when I gave the seminars here in Berkeley, also in Corvallis, and you can imagine, you go there as a kid and criticize the most important group in stream ecology on a concept they have put forward, that was really a thing. But the response I got after the seminars was, in my view, that was really something which opened up the debate. So then back in Europe, we decided to write the things down. And then we organized it as one paper with the critics...and also the critics were basically theoretical ?ballasts?

VR: Theoretical what?

BS: ?Ballasts? Things that we thought that they are so unrealistic that they are not needed. But there are many things in there that we liked very much about it. And then, the second part was on these hydraulic things and structural areas on a worldwide scale. We put all of this together into one paper. Submitted it to Freshwater Biology, I believe, and the editors cut it into two. They said "We would like to take the second part, on hydraulics," which they took, and the first part we sent in to the Canadian Journal.

VR (7:30): Well, that was a logical place to send it, because that's where the first paper on the continuum was published.

BS: Of course.

VR: What was it like going to Corvallis? I mean, you had two of the authors sitting in the audience. What was the reaction of the audience to what you said?

BS: The reaction was down to Earth, but I had a good time there, and I remember that then people who were there then visited Karlsruhe – Petersen I remember discussing things with, ?Chevelle? ?Ivenbeck?- I stayed on good terms with these people. There was not, at least at that time...we were fighting about things. About ideas. But it was not that we had bad relationships. And maybe the best proof of that was when Cummins went off from Corvallis, people encouraged me to apply for that position, to get me there.

VR: Do you remember the other criticisms, Winterbourn and Rounick, and Sam ?Lakes? which wasn't really a criticism? What do you think the reaction from them to those criticisms were relative to what yours and Higler's was? Do you have any sense, or do you remember? Because certainly Winterbourn and Rounick's was written in a very different way and tone than yours was written.

BS: They criticized...their idea was that there is not this sequence of shredders and things, and afterwards they repeatedly insisted this was the case. I still don't understand why. Nobody does. So, that was a totally different way to criticize that. It was observations and so on. Our way to do it was just to check the logic. Is it possible that this is realistic or not? It was not a thing that was basically derived from observations. So therefore, the approach of these...I don't want to say critics...and our impact to make progress in that field was different.

VR (10:18): What were your main theoretical objections to it? Do you remember just in brief the ones that twenty-five years later stick out in your mind, things you disagree with most?

BS: As far as I remember, there were predictions about the continuum, and the analogy of biological and physical systems which were unrealistic. There were things about diversity patterns, and as far as I have seen, most of what we questioned has then never been tested, so I would say our contribution to the development of the concept was that most people just said "Look this is just too unrealistic to do that." Whereas the path we took, those things we questioned in the '85 paper, I haven't seen that they have really been tested, because I think people understood that it's too unrealistic. It's just not worth to do it.

VR: I think that there have been roughly twenty-five papers that came out of the continuum project, and the second and the third papers, the ones that were senior authored by Minshall ended up doing a lot of the corrections. They had a sliding scale, for example, and I always thought that those improvements, because I think they definitely were improvements, were in response to the criticisms that they got. Did you follow those papers at all?

BS: I read them, yes, but now it's afterwards, and I am no longer concerned with being comprehensive. I don't follow it very closely. But I read the papers.

VR (12:34): If you had read that paper today, do you think you would have objected to it in the same way that you did twenty years ago?

BS: Listen, as I told you before, the reason to do that was that we were so poor that we couldn't afford to come over to the United States, and we had to organize ourselves through other funds. Now I'm so rich, I'm here at your house for Christmas and New Year, and I paid my airfare myself. So I would say reading this paper today, I would just say "Yeah, I don't agree with all of those things," and that's it. I would never write something about it.

VR: Do you think the river continuum has been good for river ecology? Do you think it's advanced management, it's advanced the science? Or do you think it moved us in a direction where people are not questioning things as much as they should?

BS: In the beginning of the 80's, what I observed was that it was cited as if it's not a concept, but as if it is reality. And then, over the time, even the big paper that they made that compared different regions of the stream the...

VR: The ecological monographs?

BS: Yeah. If you look into the most reasonable things, in the concept, which is the changes in functional feeding groups, if you look into the data, especially where they are broken down into seasons, you'll see there are some times it works, some times it doesn't work. And now, if you think about that streams are physically strong enough to get a big stone into such a small piece of sand, why do you need shredders to get leaves into smaller pieces that then collectors can profit off of?

VR (15:00): So, do you think that all in all, it was a positive contribution? That it was even better with the other ideas?

BS: This was complimentary, and it enriched the debate...outside of stream ecology, I wouldn't say it had too much impact on general ecology. But in stream ecology, it certainly had such a big impact. And I think it's good. It was good to do that, it was good to put it together. As far as I learned from the group, it was a rather long process to get it out. And they discussed, and rediscussed, and rediscussed, and with so many people involved, maybe one or the other became tired, I don't know. But, in terms of stream ecology, it was certainly a contribution that was important.

Paul Durst: You said that scientists were relying on this paper as fact in the early 80's, at least to some degree. Do you think that that attitude has changed now? Do you think they use it more as a concept like it was supposed to be?

BS: I still think some say "The river continuum concept says, therefore it is." And I don't rate the pros and cons and evidence we have for it. But many people now understand that for the articles, there are pros and cons. There are contradictions, and

still things we don't understand. For example, I still don't understand the lack of shredders in the upstream areas, I think people are working on the purposes of that, but I still don't think it's understood. Another problem, for example, to test that in Europe, was that in most parts of Europe, we don't have natural stream, so you can't test it there. And if you think about two hundred years ago, there was no forest in Europe. They used it all up to make charcoal, so if you have such a bottleneck, what does this imply for shredders and collectors and so on? There are lots of historical problems linked to the use of the landscape by the people, which is very different in Europe than the United States, for example.

VR (18:16): Let's go off in another direction. How useful do you think the river continuum has been for management of streams and for restoration?

BS: I started studying restoration in '75, and at that time, the most simple and cheapest and most effective measure was to replant riparian trees. You got shade, you got leaves, steady of banks. But that was before. You had all of these ideas that this was important. It was not formalized in a concept, but we used it to start restoration in the 70's. Then, there was this business with Block and Stanford. That was derived from the river continuum concept, and that described in theory what should happen on the downstream gradient if you put in dams into rivers. I don't know if that changed very much about administration of dams. I don't think so, but...concept-wise, I think that riparian trees, that is the most important impact for restoration .

VR (20:13): Let me ask you a question related to the popularity of the RCC. As you know, it's the most cited paper in stream ecology. What paper to you had the most effect on your career, in terms of ideas? Was it the river continuum concept, or was it a totally different one?

BS: When I studied, which was in the '70s, the amount of literature that was available was maybe 5% of what we have now in the US. So, basically, we were reading everything which was available. And it started at the beginning of the 20th century. And if you read ?Tinamind? an original, then I think that was the author, not one single paper, who had a really big impact on my thinking and my organization. Then, ?Heinz's? book.

VR: The Ecology of Running Waters?

BS: That was really something. Putting together things, the description, it was less conceptual, but it summed up what we knew at that time.

VR (22:26): Getting back to the continuum group. You've met all of them, and some of them know quite well. Do you have any feelings about the roles that different people played, or the roles that somebody who wasn't an author, who of course is now dead, so we can't ask what role he played, he was certainly there from the beginning. Do you have any sense of what the different people did?

BS: As far as I know, I cannot say what every person contributed, but as far as I was told, the basic ideas came from Vannote, who drew a lot on the geomorphology of Leopold from '64, where these geomorphologists talk a lot about energy dissipation in channels and the equilibrium between energy and channel forms, and I think that was the starting point. But then, I know that Cummins published this freshwater book in '74, it was years before they put the paper together. I don't know how much came from Cummins, how much came from Vannote via Leopold, but who ran what or did what, I don't know.

VR: Well, I want to thank you very much. You are our first interviewee.

BS: You're very welcome.